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ART. I.—*On the Effects of Opium on the Infant Subject.* By JOHN B. BECK, M.D., Professor of Materia Medica and Medical Jurisprudence in the College of Physicians and Surgeons of New York.

As a remedial agent, opium has always and justly been looked upon as one of the most valuable in our possession. It has been styled the "donum Dei," the gift of God to man, and Sydenham says of it, that it is "so necessary an instrument in the hands of a skilful physician, that the art of physic would be defective and imperfect without it; and whoever is thoroughly acquainted with its virtues and the manner of using it, will perform greater things than might reasonably be expected from the use of any single medicine."* High as this panegyric is, it is unquestionably just. Admirable, however, as this agent is, if properly used, it is equally true that, in unskilful hands, there is scarcely any article, in the whole range of the materia medica, capable of producing a greater amount of mischief. That this must be the case, is evident, if we reflect for a moment upon the nature of the effects which it is capable of producing, and, at the same time, recollect that these effects are not uniformly the same, but are modified by various circumstances. Thus, at one time, we find it operating exclusively as a stimulant, while at another it displays nothing but its sedative power. Given under certain conditions of the system, it quiets irritation, calms the pulse, softens the skin, and promotes balmy sleep. Under other conditions of the system, it quickens the pulse, suppresses the secretions, increases animal heat and disturbs the brain. In the one case, the state of the patient is materially improved; while, in the other, it is rendered worse. It

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* The Works of Thomas Sydenham, M.D., with Notes by Benjamin Rush, M.D., p. 115.

depends, then, entirely upon the circumstances under which it is given, whether it will prove salutary or injurious, and it is this which renders the proper use of it a matter which requires so much tact and experience.

Now, the circumstances which modify the action of this agent are numerous. Age, sex, temperament, climate, nature of the disease, stage of the disease, in short, everything which can affect the condition of the system, modifies, in a greater or less degree, its effects. To understand, therefore, completely the manner in which it operates on the system in its different phases, it is necessary to analyse all these modifying circumstances. On the present occasion, I propose to make a few remarks upon only one of them, viz., *age*, with the view of showing more particularly the peculiarities of its operation on the young subject.

With regard to the effects of opium on young subjects, there are two facts which seem to be well established. The *first* is, that it acts with much *greater energy* on the infant than it does on the adult; the *second* is, that it is more *uncertain* in its action on the infant than the adult. It is in consequence of these peculiarities attending its operation on the infant, that even the smallest quantities have not unfrequently produced the most unexpected and even fatal results. Of this, almost every physician must have seen some melancholy instances. Dr. John Clarke states, that "half a drachm of genuine syrup of white poppy, and, in some instances, a few drops of Dalby's Carminative, have proved fatal, in the course of a few hours, to very young infants."* In one case, he says, forty drops of Dalby's Carminative destroyed an infant. Mr. Marley says, "I have known three or four instances where the most dangerous symptoms were produced by Godfrey's Cordial and Dalby's Carminative; two nostrums which have no doubt added considerably to the mortality of infants." In a case that fell under his observation, the most rapid and alarming symptoms followed the exhibition of an ordinary dose of syrup of poppies. In another case, he knew half a small tea-spoonful of the syrup of poppies prove nearly fatal to a child eight or ten days old. Thirty-five drops of Dalby's Carminative, he has known to prove fatal to a young child, while, in other cases, larger doses have been given without any unpleasant effects. The same writer relates the case of an infant, nearly poisoned, by considerably less than half an ordinary-sized tea-spoonful of paregoric.† Dr. Bard says, he once knew an infant of several months old killed by ten drops of laudanum, and another brought into very great danger by less than two drops.‡ Dr. Montgomery states, that he has known more than one instance in which a tea-spoonful of the syrup of poppies has proved fatal to a healthy child.§ Professor Hamilton relates two cases, in which four drops of laudanum proved fatal to children some months old.‖ Dr. Merriman reports two cases, in which a dose of Godfrey's Cordial proved fatal.¶ He also states, that he once saw a child in the month thrown into a state of excessive stupor, by taking one dose only of a mixture in which there

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* Commentaries on the Diseases of Children, p. 33.

† On the Diseases of Children, pp. 29, 30, 31.

‡ Bard's Midwifery, p. 328. § Pereira's Mat. Med., vol. ii., p. 711.

‖ A Treatise on the Management of Female Complaints and of Children in Early Infancy, p. 344.

¶ Treatise on the Diseases of Children. By M. Underwood, with Notes by Merriman, Hall, and Bell, p. 106.

were four drops of laudanum; the actual quantity swallowed could scarcely have amounted to one drop.* Dr. Cristison states, that "the administration of three drops of laudanum in a chalk mixture for diarrhœa, to a stout child, fourteen months old, was followed by coma, convulsions and death in six hours." In another infant, a few weeks old, death resulted from taking four drops of laudanum.† Dr. Ryan states, that he has known one drop of the "sedative liquor of opium" narcotize an infant‡. Of laudanum, two drops have been known to kill an infant, nay, in one case, a single drop destroyed a new-born infant.§ I have myself seen a young child narcotized by about twenty drops of paregoric. The foregoing facts are sufficient to show that opium acts with peculiar energy and uncertainty upon the infant subject. The causes of this would seem to be the following:

In the *first place*, the great difference in the physical organization of the *infant* and the *adult*. In the young subject the brain and nervous system are much more impressible, and the slightest causes, as we know, will sometimes derange them. Besides, in the infant, the circulation is more rapid—there is a greater proportionate quantity of blood circulating in the brain, and hence a much greater tendency to cerebral determinations. From these peculiarities in the organization of the infant, it happens that convulsions are so much more common in the early periods of life. Thus, for example, the irritation of teething—of worms or crude matters in the intestines, is frequently followed by convulsions. Intermittent fever, which in the adult commences with a chill, in the child is frequently ushered in by a convulsion. Scarlet fever, too, in the child, not unfrequently commences with a convulsion, while in the adult I have never witnessed such an occurrence. Now, with such peculiar predispositions characterizing the system in infancy, it may readily be conceived how it is that such an article as opium should act with more power at that period than in after life.

In the *second place*, the difference in the *temperament* or *constitution* of infants. In the adult, we know as a matter of fact, that opium differs greatly in its effects in different constitutions. Thus, as a general rule, the sanguine temperament does not appear to bear the use of this drug as well as the melancholic or the nervous. In the former, it is much more apt to produce cerebral disturbance, and in large doses is more likely to prove injurious. Now, infants differ from one another, as much, if not more, than adults, in these peculiarities of constitution, and, as a matter of course, the difference in the effects of this article must be greater. Besides, as these peculiarities and differences can only be detected by actual experience, and as we cannot of necessity have the same benefit of experience in the case of infants, it is obvious that the difficulty of justly appreciating the action of this drug on the infant must be greatly increased. A greater or less degree of uncertainty, therefore, must necessarily from this cause attend its use in the early periods of life.

In the *third place*, the actual state of the system as to disease. There is no circumstance which modifies the effect of opium in so great a de-

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* Treatise on the Diseases of Children. By M. Underwood, with Notes by Merriman, Hall, and Bell, p. 143.

† Treatise on Poisons. Second Edition, p. 625.

‡ Midwifery, p. 477. § London Medical Gazette for 1839, p. 294.

gree as this. In the adult, we see this continually. In some conditions of the system, even small doses produce the most unpleasant effects, while in other conditions, immense quantities may be given with little or no effect; thus, for example, when severe pain or spasm are present, quantities of this article can be borne, which under other circumstances would prove exceedingly injurious. As illustrative of this, I quote an interesting case related by Dr. Percival. He states that a young man was admitted into the Manchester Hospital, on account of a violent spasmodic disease which recurred periodically in the evening, and after trials of various remedies, doses of opium sufficiently large to mitigate the violence of the paroxysm were ordered, and he took twenty-two grains every night during a week, without producing any soporific effect. On the eighth night he had no return of the spasm. He nevertheless took the opium and in the morning was found dead.* In this case, a great and sudden change had unquestionably taken place in the nervous system of the patient, and to this must be ascribed the difference of effect. If in the adult, the state of the system makes such a wide difference in the effects of this article, how much more so must all this be the case in the sensitive infant; and it is by not duly regarding this, that such unexpected results sometimes follow from the use of opium. Thus, for example, a child laboring under the acute pain of colic will tolerate doses, which, in the ordinary condition of the system, might prove destructive.

There is one condition of the young subject particularly in which this remedy is frequently resorted to, in which this is strikingly illustrated. I mean that state of exhaustion which arises from diarrhœa and other bowel complaints. In this state the head is very apt to become affected, and if opium be given with a view of checking the intestinal discharges, not unfrequently insensibility gradually creeps over the little sufferer, and in a short time death is the result; and this, too, even when the quantity used has been apparently adapted with great nicety to the wants of the case. Every observing practitioner must have witnessed such instances. Now in many cases of this kind, there can be no question that the child sinks under the sedative influence of the opium; and the reason is that, in the exhausted state brought on by the disease, the system succumbs much more readily to the narcotic effects of this article, than it does in other conditions of the system.

The foregoing considerations appear to me sufficient to account for the greater power, as well as uncertainty in the action of opium on the infant, than on the adult.

If it be a fact, then, that opium acts in this way upon the infant, it appears to me to involve inferences of great practical moment, which cannot be too deeply impressed on the mind of the young practitioner.

1. That its use should *be avoided as much as possible* in the young subject. I will not say that it ought to be interdicted altogether, because if used with discretion, it is a remedy of great value in many of the diseases of infants, but it should never be used unless there exists a strong and manifest necessity for it.

2. Great caution should be exercised in the form in which it is administered. No preparation should ever be used, which *is not of a known and determined strength*. In England, the *Syrup of Poppies* is

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* Percival's works, vol. i., p. 422.

the preparation most used for children. In this country it is also used, but not to the same extent. This is a pleasant and mild opiate, and on these accounts is well adapted for children. It is liable, however, to great objections. Besides being apt to ferment and spoil, it is very *variable in its strength.** On this account it is really a very dangerous article; and many cases are recorded (some of which I have already related) in which fatal results have followed the use of it, even in moderate doses. Another objection is, that it is liable to sophistication. Thus a mixture of *laudanum and simple syrup*, has sometimes been sold for it. In the London Medical Gazette (May, 1831, p. 253), a case is related, where a child died in consequence of a small dose of this latter compound having been given by the mother, who had previously given the same quantity of the pure Syrup of Poppies, with advantage.

The best preparations for children are *laudanum*,† and *elixir paregoric*. These are of known strength, and susceptible of division into the minutest doses. *Dover's powder* is another preparation, which may be given to children. It may readily be divided into the smallest doses, and it seems to act much more mildly than equivalent doses of simple opium. It need hardly be stated, that all such articles as Godfrey's Cordial, Dalby's Carminative, &c., should be totally discarded from regular practice. Besides being uncertain in their strength, and on that account exceedingly objectionable, the sanction thus given to them encourages their use by persons out of the profession, who cannot be supposed to be acquainted with the dangerous effects of opium on the infant system.

3. In very young subjects, we should never begin the use of this article, except in *very small doses*. Although most practical writers lay down cautions about the use of opium in these cases, yet it does not appear to me that these cautions are sufficiently precise. Most of the writers to whom I allude, specify doses as suitable to certain ages, without stating, that even these doses may, in certain conditions of the system, prove just as injurious as much larger doses. To illustrate, I will quote the directions given by one of our standard authorities. Dr. Dewees says, "the proper dose of laudanum for infants and children, may be reckoned at the following rates. Half a drop for a child under ten days old; a drop, for one from that period to the end of the month; a drop and a half, or two drops for one from that period to three months; three drops from this to nine months, &c., &c." "When laudanum is to be used as an injection, we may safely increase the quantity three or four fold." He adds, "These doses are prescribed for children who are altogether unused to the use of this drug; the power of bearing more, may be rapidly increased by habit."‡ Now, it appears to me that a more

* When properly prepared, *one ounce* of the Syrup of Poppies is generally estimated to contain about one grain of opium; a drachm, therefore, contains ¼th of a grain. From the variable proportions, however, of active principle contained in the capsules of opium, it is impossible to calculate with any degree of precision, the strength of the syrup prepared from them.

† Even this preparation should not be given without circumspection. "When long kept, with occasional exposure to the air, laudanum becomes thick in consequence of the evaporation of a portion of the alcohol, and the deposition of opium. If given in this state, it often acts with unexpected energy, and cases of death have resulted in infants from its use in doses, which would have been entirely safe if the tincture had been clear."—*Wood & Bache's Dispensary, p. 1032, 2d ed.*

‡ On the Diseases of Children, p. 363.

dangerous set of directions could not well have been given. Although many children may bear the quantities here specified without injury, yet every now and then a case will occur, in which the most serious results will follow; and it is against these that the necessary precautions should always be taken. In the case of a new-born infant, we are entirely ignorant of the manner in which such an article as this will affect it, and it therefore will not do to begin with *average* doses. To practise safely, we must feel our way with doses much smaller; and then we shall have some guide, and the only guide which the nature of the case admits of, to make the necessary increase in the quantity to be given. Under no circumstances, as a first dose, ought half a drop to be given to a child under ten days—or a drop to a child during the first month. One eighth of a drop is sufficient to begin with. The quantity, too, directed for an injection is too large. Instead of three or four times the quantity given by the mouth, as far as my experience goes, double the quantity is quite sufficient.

4. The doses of opium should not be repeated *at too short intervals*. This, too, is a point which is not sufficiently guarded by some practical writers. One writer, for example, after specifying the quantity suitable for a child of two and three months, adds, that "this is not to be repeated in less than an hour." If this means anything, it means of course, that after the lapse of an hour, the dose may be repeated with safety. This, however, will not be sustained by experience. Even if a first dose does not narcotize, it frequently produces a degree of listlessness and indifference to food on the part of the child, which, if it be kept up by repetitions of the opiate, may eventually prove just as destructive. This is strikingly illustrated in those states of exhaustion from diarrhœa, where the due supply of nourishment is so essential to recovery. Where repeated opiates are necessary, the intervals between the doses should be long enough to enable the child to recover somewhat from the sedative influence.

Before concluding these observations, I cannot refrain from making a remark or two in relation to the use of this article by persons out of the profession. The mischief that is done in this way is incalculable. If, in the hands of those acquainted with its virtues, opium is an article so dangerous and uncertain in its action, what must it be in the hands of the ignorant; and yet we see it given to infants, day after day, and night after night, by nurses and mothers, not merely without the consent of the physician, but sometimes contrary to his express injunctions.

There are two ways in which it is used by persons out of the profession, in both of which it proves injurious to the child. The first is by giving it in occasional doses; the second, by giving it constantly. The first is bad enough, but the second is still worse. The first, now and then, unexpectedly destroys a child; the second is followed by a train of the most disgusting consequences, worse, if possible, than those of habitual drunkenness in the adult. Fortunately, these latter cases are not of such frequent occurrence; occasionally, however, they are met with where the parent, for the purpose of quieting it, has been induced to keep a child for months under the daily influence of paregoric, Godfrey's Cordial, or some other opiate nostrum. In these cases, the effect is to stunt the growth of the child; it is emaciated and puny; the skin is flabby and shrivelled; the lips are bloated, and the countenance sallow and wrinkled. There is an absence of all intelligence, and the whole appearance

is haggard and aged, presenting a sort of "miniature of old age." Not long since I witnessed a case of this kind, in which a child of fourteen months old did not appear larger than one of two or three months. With the exception of one month, it had been kept upon paregoric almost every day since its birth. The mother was a poor woman, and on inquiring of her the reason, she stated that she had resorted to this method of keeping the child quiet while she attended to her work.

Of the extent of the mischief annually perpetrated by the unprofessional use of opium, some idea may be formed from a report made to the House of Commons, containing returns from the coroners of England and Wales, of the inquisitions held by them during the years 1837 and 1838, in cases of death by poison. The total number of deaths by poison in these years was 543, of which 52 were very young children, most of them at the breast, in consequence of opium, or some of its preparations having been given by mothers and nurses, in ignorance of its effects. In addition to this, 20 more were destroyed by opium or laudanum administered in mistake for other medicines.*

These facts are certainly appalling; and if any subject connected with medical police is worthy of the attention of the public authorities, it is certainly this. How the evil is to be corrected, it is not easy to say. Much, however, might be done by the proper dissemination of information on the subject. In most cases where opium is administered to children by persons out of the profession, it is disguised in the shape of some nostrum, so that they are not aware of what they are giving, and even when they are aware of it, they are not acquainted with the dangerous effects of it on the infant system. If parents and nurses were made better acquainted with the fact that such articles as Godfrey's Cordial, Dalby's Carminative, and most of our medicated lozenges and candies, owe all their composing properties to the opium which they contain, and that opium even in small doses is frequently a deadly poison to the infant, one would suppose that it could not but exert a salutary influence in correcting, to a certain extent at least, the evil of which we are speaking, and the dissemination of this kind of knowledge by the proper authorities would confer a lasting benefit upon the community.

I have thrown together the preceding observations because the subject is one of interest and importance, and because it appears to me that it has not sufficiently attracted the attention of the profession.

ART. II.—*Case of Removal of Uterine Polypus.* By JOHN V. P. QUACKENBUSH, M. D., of Albany.

I WAS called to Miss ——, ætatis 39, June 4, 1843, whom I found much exhausted by a severe hemorrhage, which had existed for a number of days. She told me she had not enjoyed good health for two or three years, during portions of which time she had been under medical treatment; that every month she suffered much from excessive flooding,

* See Dunglison's Intelligencer, vol. iii., p. 299.

every successive turn of which rendered her still weaker; and that she had now given up all hopes of ever getting well.

On making an examination per vaginam, I found the uterus in the following condition. The neck was entirely obliterated, as at the ninth month of pregnancy, and it gave the same sensation to the touch as when the membranes have been prematurely ruptured, and the head of the fœtus presses directly upon the walls of the uterus. The os uteri was dilated one inch in diameter; and upon inserting my finger, I discovered a hard tumor, which I pronounced to be a polypus. I now inspired my patient with hopes, by stating that I thought I would be enabled to remove it during the course of the summer. I endeavored to suppress the hemorrhage by means of astringents and injections, but did not effect much by my treatment. In the course of a few days, when the hemorrhage had ceased, I prescribed muriated tincture of iron, drops twelve, morning, noon and night, and the daily ablution of the parts with cold water. Under this treatment my patient gained much. She became stronger, regained her appetite in a great measure, and was enabled to sleep well, which she had not been able to do for many months. She continued to improve until August 20th, when she had another very severe attack of menorrhagia, accompanied with regular labor pains. I visited her, and, on examination, found that the polypus had descended from the uterus and now occupied the vagina. I decided upon operating; and not being able to obtain any suitable instrument at the time, I constructed one for myself (a diagram of which accompanies this, and which has at least one recommendation—simplicity). I applied my ligature, August 26th. During the time the ligature was applied, the patient appeared to grow weaker daily, and I was compelled to support her strength by administering stimulants. On the sixth morning, Sept. 1, the ligature came away; and, upon examination, I found the polypus detached and lying loose in the vagina.

In the evening a messenger came after me, wishing me to visit my patient immediately, as she was much worse. I found her suffering severe pain, caused by the efforts of the vagina to relieve itself. The polypus was expelled during the evening; the patient fell asleep and slept soundly till morning, when she felt much refreshed, and appeared better than I had seen her for a month previously. Since that time, she has been gradually improving; and she is now able to sit up during the greater portion of the day.

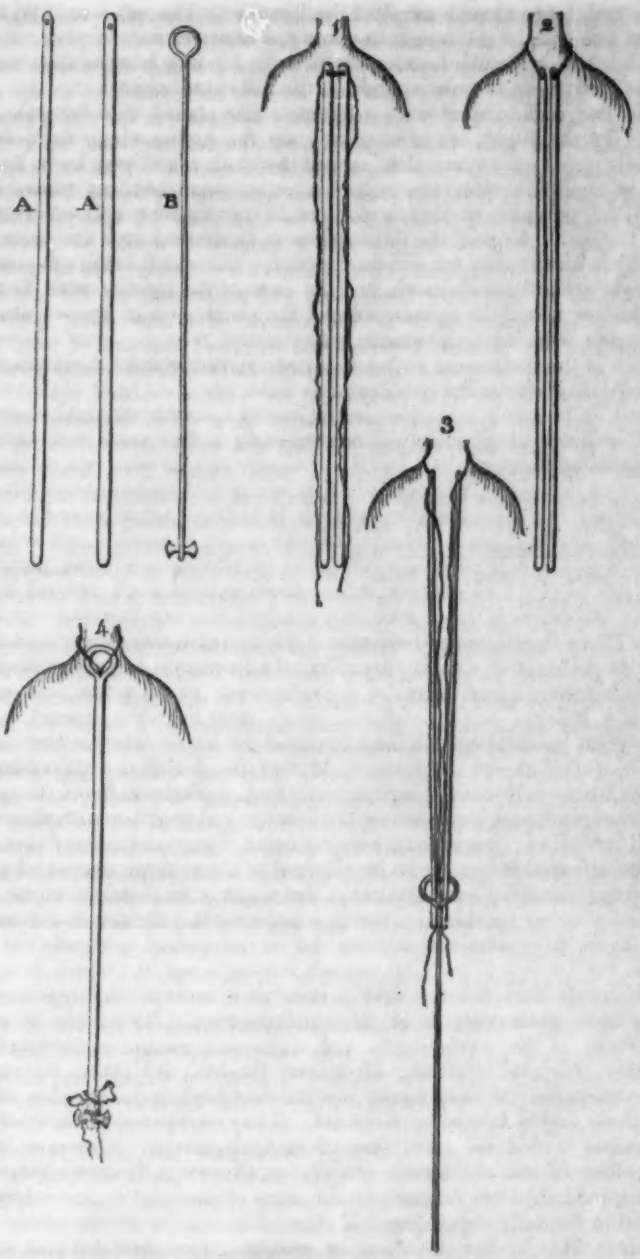
The tumor, after being deprived of all the blood it contained, weighed ten ounces.

At the time I operated, there was another tumor, of the size of a goose-egg, connected, as I supposed, with the left ovarium, which had existed for more than a year. On Sunday, Sept. 3, my patient had a discharge from the vagina, of a substance which, from the description she gave of it, I considered to be pus.

While the discharge continued, the tumor *seemed* to diminish; and this morning, Sept. 18, upon examination, I could not feel it, except when I pressed very hard upon the walls of the abdomen. It does not now appear larger than a filbert.

Upon the nature and situation of this tumor, I will leave you to decide for yourself.

The instrument I used, consists of three small rods, about eight inches in length and perfectly straight. Two of the rods (AA) have a small hole



AA—Two rods, 8 inches long, 1-12 inch in diameter.
B—Rod of same dimensions, having a loop or ring to pass up over the other two, with its shoulder for fastening the ligature.
The instrument can be constructed either of silver or steel.

at one end, large enough to admit the ligature. The other rod (B) has a loop at one end, large enough to allow the first two rods to pass; and, at the other end, a shoulder, around which the ligature is to be fastened.

The instrument is to be applied in the following manner:

The two rods, armed with a ligature, are placed side by side, and, guided by the finger, are to be passed up the vagina, along the polypus, till their upper ends reach that part of the stalk round which the ligature is to be applied. Now the rods are to be separated and moved quite round the polypus, so that a portion of the ligature will encircle the stalk; then the loop of the third rod is to be slipped over the other two, by which means they are drawn together: all which being effected, the two rods are to be withdrawn, and the ends of the ligature with them.

You now fasten the ligature around the shoulder, and leave nothing in the vagina with it but the simple rod.

Such is the instrument I wish to introduce, and which I think possesses two advantages over the one generally used.

First, it is much more simple and easily constructed; and, secondly, as it is much smaller, it will consequently cause much less irritation during its application.

ART. III.—*Pyretological Inquiries. No.* 1. *Gastro-enteritis; as a cause, or concomitant of Fever.* By WM. C. ROBERTS, M.D., Physician to the Northern Dispensary.

THE great medical revolutionist of modern times, enumerated, as the basis of the theory of fever held by the so-called "physiological" school, three celebrated propositions: first, Inflammation of the gastric mucous membrane, is *never* met with in the cadaver, but with that of the small intestines; it is then far better to call it "gastro-enteritis:" secondly, all the *essential* fevers are to be referred to a simple or compound gastro-enteritis: thirdly, Fever is always the result of an irritation of the heart, primitive or sympathetic. We thus perceive that Broussais did not refer *all* fevers to gastro-enteritis as he is supposed generally to have done.

Broussais was not the first person who asserted the dependence of fever upon gastro-enteric, or other inflammation. The labors of certain observers of the seventeenth and eighteenth centuries, Roederer and Wagler, Michael Sarconi, Morgagni, Baglivi, and others, appeared to have prepared the way for an inevitable reform in the doctrine respecting those fevers said to be essential. They tend in fact to embrace those affections within the great class of the phlegmasiæ. However, toward the close of the eighteenth century, an illustrious French nosographer, Pinel, published his famous classification of essential fevers, which consisted in formally separating this class of diseases from that of the phlegmasiæ. He divides the class of primitive, or essential fevers, into six orders: viz., inflammatory or angiotenic; bilious or meningo-gastric; mucous or adeno-gastric; putrid or adynamic; malignant or ataxic; adeno-nervous or typhus, properly so called, and plague. The work of

MM. Petit and Serres, published in 1813, was likely, as it appeared, to be the promoter of a profound pyretological revolution. Such was not, however, the case; and far from thereby invalidating the reigning doctrine, the now venerable Petit, not daring to decide whether the disease which he was describing was new and evanescent, and misunderstood before his time, separated it positively from the numerous and varied classes of adynamic fever, with which, by an error, he says, common to him and all other practitioners, he had previously confounded it. Petit recognized in entero-mesenteric fever, an affection sui generis, distinct from all which had previously been described. To the author of the Examen des Doctrines, etc., published in 1816, was reserved the marked honor of accomplishing the great work of the localization, or rather the dissentialization of fevers, and of overturning, from top to bottom, the system of Pinel. A new pyretological era, then, dates from the publication of this remarkable work.

Among the most energetic of the opponents of Broussais, is Chomel, who, in his treatise on the typhoid fever, regards the intestinal ulcerations which are met with in that disease, as being very often the *effects*, and seldom *the cause* of the symptoms which are observed. Chomel is generally supported by Louis, and ably opposed by Bouillaud, an enthusiastic and very clever disciple of the physiological school. Andral, the sagacious and philosophical, in the first edition of his Clinique, avoided committing himself positively on the grand question of the non-essentiality of fevers; but in later editions of that inestimable work, he declares that the progress of science had induced him to devote a volume specially to fevers, and that, interpreting otherwise than he had done, cases of that disease, he had arranged some under the phlegmasiæ of the nervous centres, and others among those relating to the inflammations of the abdominal viscera. "Such was," says Bouillaud, "the opinion which I had myself developed in my Clinical and Experimental Treatise on Essential Fevers, 1826." It seems, however, to be only just to the memory of Pinel, to declare that while he advocated the doctrine of the essentiality of fevers, and carefully laid down the distinctions between those which are primary and those which are secondary, he nevertheless assigned to each species of primary fever admitted into his classification, a concomitant local disturbance. He held that inflammatory fever was connected with disturbance in the general circulatory system; bilious fever, with disorder in the digestive organs; a particular form of gastric fever, with disease in the intestinal mucous follicles; nervous fever, with derangement of the brain and nervous system; and typhoid fever, with depression of the vital powers; thus, obviously, in some measure localizing the disease.

After this brief historical account of the theories of fever, dating from the publication of Petit and Serres, we proceed to inquire into the correctness of Broussais' assertion, that *all* the *essential* fevers are to be referred to a simple or compound gastro-enteritis. That is: whether in all essential fevers, we may expect to meet with inflammation either of the stomach or intestines, or both, of whatsoever nature. In order to ascertain the views of the writers of the present day, in reference to this subject, we will begin, without particular regard to chronological succession, with the opinion of Louis. This author asserts that in all the cases he has noted under the general name of fever, an intestinal lesion was constant. He adds: "we cannot call the typhoid affection a

gastro-enteritis, for the mucous membrane of the stomach was more frequently in a natural condition after the typhoid affection, than in subjects who died of any other acute disease." He has also given some cases where the anatomical appearances were deceptive, *i. e.*, where those which had been expected did not appear. Dr. Stokes asserts, that we "cannot affirm that every continued or typhoid fever, implies the existence of a gastritis;" but that the alteration of the small intestines is much more frequent. And he elsewhere says, that "observations formerly cited do not permit us to doubt, that in the human species, symptoms completely similar to those of dothinenteritis, may occur without it, and without *any* lesion of the digestive tube." Dr. Gerhard, of Philadelphia, says that "the triple lesion constituting the anatomical character of typhoid fever, did not exist in the epidemic fever." Chomel tells us that Louis and Andral show, that in several cases in which, during life, the symptoms had been exactly those of typhoid fever, yet there were none of the alterations proper to this malady, *nor any others* to which the symptoms could be referred." He assents to the wish of Louis, that of such cases an indeterminate affection should be made. Doctor Christison, of Edinburgh, has given us the results of his observations on this subject. Before we cite them, we will quote some remarks of his, bearing on the general subject with which we are engaged. "Local inflammation in particular, has appeared to some modern cultivators of pathology, to be so invariably an attendant on what are called primary continued fevers, that they have been led to call in question the existence of any *true* primary fevers, and to maintain that fever of every kind is constantly a sympathetic affection; a phenomenon secondary to inflammation in some special organ. The doctrine here alluded to, which has gained not a few converts in Great Britain, and which, on the continent, but especially in France, seems at the present time to predominate, *is of so much consequence*, both as striking at the very root of the theory of fever previously current, and also as involving practical precepts of high importance, that a necessity arises for considering the subject in some detail. And the necessity becomes not the less urgent, if it be true, as appears highly probable, that the doctrine in question, however specious and widely disseminated, is in reality untenable; being founded, like many other hallucinations in physic, upon narrow, though as far as it goes, correct enough observation. There can be no question that the essentialists who looked almost entirely to fever in the abstract, neglected too much the local affections which attend it, and to which so much importance has been attached in modern times." He then states that examples of the three forms of continued fever, synocha, synochus, and typhus, occasionally present themselves without any appreciable sign of local inflammation during life, or any corresponding alteration after death. Again, he says, "gastritis and muco-enteritis are undoubtedly met with in the fevers of Britain. Much more frequently, however, both are absent, and the common rule is, that a case of fever passes through all the stages of the disease, without any symptoms whatever referable to disordered bowels, except a little flatulent distension, vague uneasiness, and constipation. But in particular, diarrhœa, the most invariable symptom, both of the milder and more severe disorder, is comparatively a rare incident; at all events, far less common than the very opposite condition." Again: "There can be no question that cases of death from the continued fever of the typhoid, or synochus type, occasionally present themselves where no *morbid appearances what-*

ever are detected, except congestion of internal organs. In the epidemics of Edinburgh, it is rare to find any unnatural condition of the intestinal mucous membrane, so constantly observed in France; frequently the entire tract of the alimentary canal is quite healthy." "When we remember," says Dr. Tweedie, "how many cases of pure fever have been examined by good anatomists in this and other countries, and even in the very place from which the notion first originated, and no unhealthy appearances discovered, I confess I am inclined to regard this condition of the mucous membrane (inflammatory, according to Broussais), as one of the many complications of this inscrutable disease." In 523 cases of fever, mentioned by this author, thirteen presented no disease in any part of the intestinal canal, or in any of the abdominal viscera. Dr. Roupell, in a "short treatise on Typhus fever," observes—"I have met with several cases in which no morbid change was perceptible to our ordinary methods of investigation." And again: "How indeed can M. Broussais' theory be admitted, that typhus is produced by gastro-enteritis, when we find that many cases terminate either by death or recovery, without any symptoms referable to the bowels in the one case, or any morbid alterations in the other?"—Am. Ed., p. 118.

Andral, in the first edition of his Clinique, divided fever into three classes; in the third of these classes, he places cases in which the typhoid symptoms were observed *without* lesions of the digestive tube. In them, as in follicular enteritis, it is, he says, in the nervous centres, secondarily affected, that we must seek the cause of the severity of the disease; it is in the modifications which *they* have undergone, that we must seek the explanation of the symptoms. In one case mentioned by him, the symptoms of which, however, were more ataxic than adynamic, the only lesion of the digestive tube consisted in numerous ulcers in the stomach. Again he says: "In the pyrexiæ, which constitute the different morbid groups designated in Pinel's Nosography by the name of essential fevers, we do not always find after death lesions of the digestive tube." Respecting the stomach, he says, "it is found healthy in a considerable number of persons who die during what is called essential fever, whatever may be its nature, or the symptomatic form of it. The changes found in the stomachs of persons who die during the progress of this fever, have nothing peculiar; nothing which can constitute their anatomical character. These changes do not differ from those discovered in the bodies of persons who have died of other diseases, whether acute or chronic. They are met with in nearly equal frequency, both in those who have died with continued fever, or of any other disease." "Every essential fever then," he concludes, "is not necessarily the product of gastritis. The traces of gastritis found on opening bodies, cannot always suffice to explain the different morbid groups called essential fevers." But he also admits that there are cases, however, where acute gastritis should be considered as the cause of typhoid fever. "Primitive acute follicular enteritis," says Cruveilhier, "is not an enteritis properly so called, *much less is it a gastro-enteritis*, the stomach being almost always healthy in that disease; one may in vain ingest by the mouth or by the anus, all the acridities, all the stimulants imaginable; follicular enteritis would never be produced by them."

Resting upon these most respectable authorities, it follows from the quotations made, that in many cases of essential fever, there is neither gastro-enteritis, nor even separate lesions of the stomach or intestines;

and, therefore, as also for the reasons assigned by Andral, we must dissent from the correctness of the 2d of the propositions assumed by the celebrated Broussais, and consider as erroneous the very postulate, upon which is built the whole superstructure of the "physiological" doctrine of fever.

If, then, as appears to be established, *all* the essential fevers cannot be referred to a simple or compound gastro-enteritis, let us next inquire whether we can have an essential fever, in which no marked lesion of any organ shall be discoverable after death? "If," says Chomel, "in the actual state of science, *when no appreciable lesion shall be discovered*, or when those which are observed are not sufficient to account for the gravity of the symptoms, we are necessarily led to admit a concealed alteration either in the nervous system or animal fluids." "Shall we not," says Andral, "be warranted in saying that the symptoms called adynamic or ataxic, are the results of a disturbance in the action of the nervous system; a disturbance sometimes primary, *and which may then exist without a lesion appreciable on opening the body*, but most frequently consecutive," &c. On the strength of these two citations, we derive the conclusion that essential fevers may exist without the presence of lesions appreciable after death; and as a corollary from all that has preceded, we may assert, that to designate such fevers by the name of "gastro-enteritis," is inaccurate.

In defence of the school which he so warmly supports, Bouillaud observes: "If it is true that the ileum alone may inflame, it is not less true that the stomach, duodenum and jejunum may also inflame isolatedly. These inflammations will have symptoms different from those of the inflammations of the ileum, and will *constitute* the diseases formerly designated under the names of gastric, or bilious simple fevers; and as, on the other hand, there are cases in which the upper and lower portions of the small intestines inflame, simultaneously, successively, or conjointly with the stomach, those mixed fevers will result, designated under the names of bilioso-putrid, gastro-adynamic fever," &c. "It would," he adds, "be a great error of observation and reasoning (I mean of the most exact reasoning), not to localize with Broussais and Pinel himself, the gastric and bilious symptoms in the stomach and its annexes." . . We perceive that in this extract, Bouillaud asserts the gastro-enteric origin of some cases of fever; but this connection is not proved or said *always* to exist; nor if it were so, is it proved that the inflammations are the causes rather than the consequences of the fever. It would, indeed, be a grave error to deny to such fevers as really do co-exist with gastro-enteric symptoms, a gastro-enteric connection; but we have seen that such connection does not *always* exist, and as the symptoms of bilious and gastric derangement are often present during life, without leaving behind them traces of an inflammatory action in the stomach, it becomes an additional argument against even that localization which Bouillaud maintains to be often necessary in sound reasoning and observation.

The 3d inquiry, to which we shall briefly allude, is this: may there, in fever, exist enteritis without gastritis, or gastritis without enteritis? Now, the very quotation we have just made from Bouillaud, affirms this. Louis distinctly states, in speaking of the fever which is attended with glandular ulceration, that its cause is not always to be found in any appreciable *lesions of the stomach*, whereas the *intestinal* lesion was constant. He adds, that he considers the lesions in the stomach which

are met with, as only secondary; and asserts that no gastric symptom by itself, can aid us much in the diagnosis of lesions of the stomach. We cannot, then, in every case, subscribe to the opinion of Bouillaud, that "the gastric symptoms, properly so called, when well pronounced, seem to be the expression of a *truly inflammatory* state of the mucous membrane of the stomach, either simple or combined with a similar state of the upper part of the small intestines, with reaction on the biliary apparatus (bilious fever)." And we glean from the answer to the query proposed, an additional argument against the use of the term "gastro-enteritis" as a synonym of essential fever. Let us further add, that the facts now stated, invalidate the correctness of the first of the propositions asserted by Broussais, that "the inflammations of the gastric mucous membrane are *never* met with in the cadaver, but with those of the small intestine."

These arguments, which appear to us to be conclusive, are four: 1st, Gastro-enteritis is not met with in all cases of essential fever; 2d, In some cases no marked lesion whatever is appreciable; 3d, Gastritis or enteritis may exist isolatedly in such cases; 4th, The gastric symptoms met with in fevers, are not necessarily connected with marked lesions of the stomach itself. In corroboration of the last remark, let us adduce the testimony of a sensible and tolerably disinterested physician, of much experience in the disease in question, Dr. Tweedie. "There are no symptoms which can be depended on to indicate the existence of gastro-enteritis in fever." We have taken the pains to analyse the symptoms in eighteen cases of typhus or typhoid fever, contained in his Clinical Report, and find that in four, presenting tenderness of the belly or epigastrium, retching and relaxation of the bowels, no such lesions were met with; while in several in which they existed extensively, no abdominal symptoms appeared. And these results correspond with what all writers have stated, and could, if it were necessary, be substantiated by an abundance of examples. A single remark from the very highest medical authority extant, may be introduced in further confirmation. "Very often," says Andral, "we have been unable to establish any relation between the intensity of these lesions and the severity of the symptoms, so that, for instance, in the digestive tube of an individual who died during the course of a slight bilious fever, we found an alteration equal in its nature and intensity to that which we detected in the intestine of another who died with the very worst symptoms of an ataxo-adynamic fever."

But if we have shown that we are not to look for lesions of the gastro-intestinal mucous membrane, in every case of fever; and if, moreover, the symptoms may exist while the anatomical lesions are themselves absent, are we, on these accounts, to attach less importance to the fact, that they are of very frequent occurrence? Far from it. If, on the one hand, the non-essentialists have overlooked too much the importance of the general febrile state, the influence of the *materies morbi* in the production of the local lesions, and raised too high their importance as the exciting cause of the disease in question, induced to do so by an excessive confidence in the visible indications of pathological anatomy; the essentialists, on the other hand, have looked too much to fever in the abstract, and too much neglected the local inflammations which attend it. Our choice must be a prudent eclecticism. Let us bear in mind the second corollary of Andral. "In the pyrexiæ, which constitute the different morbid groups, designated by the name of essential fever, lesions

of the digestive tube are found very frequently after death, in ninety-eight cases out of a hundred." "The frequency of intestinal lesions in fever," continues this conscientious and philosophical author, "has so convinced me of their importance, that although I know them to be sometimes wanting, and also, that when they do exist, they cannot explain everything, still, I have thought it right to speak of fevers in that part of my Clinique, which is especially devoted to the diseases of the abdominal organs." Having divided fevers into three classes, the second of which comprises those in which, fewer in number, the digestive tube presents, not an affection of its follicles, but a simple inflammation of its mucous membrane, characterized by redness and softening (the gastro-enteritis of Broussais), he adds: "We shall prove that it, as well as follicular enteritis, may give rise to typhoid symptoms, but in a smaller number of cases." Respecting the possible occurrence of all and each of the lesions of the digestive tube in fever, while, on the other hand, we must remember that they may exist without it, and it without them, we may adopt the language of a staunch disciple of the physiological school—Bouillaud. "Admitting," he says, "as individual and perfectly distinct inflammations, gastritis, duodenitis, enteritis of the small intestines, comprising jejunitis, and ileitis, enteritis of the large intestine, which may occupy either all the large intestine, or some portion of it only, nevertheless, the most exact observation compels us to acknowledge, that, in a great number of cases called typhoid fever, or affection, all these different phlegmasiæ, or only some of them, may exist simultaneously, and in different degrees, not ordinarily the same for each." Anxious to lay before the reader an impartial view of this great medical bone of contention, and to impress upon him the necessity for close observation in the cases which come under his own care, we shall quote a passage from the work of the distinguished Chomel, relating to the value to be attached to the pathological appearances of the stomach. "The characters of red coloration of the stomach, which persists after inflammation in the cadaver, have not been established in a manner sufficiently precise for us to pronounce on simple inspection, whether redness is of an inflammatory nature, or whether, on the contrary, it depends on other causes. The same remarks apply to the slate-color. We are obliged, then, to recur to the symptoms which for a few days preceded death, to decide upon the existence of gastritis." The same author, a sagacious physician, but a bitter opponent of the school, and we imagine a personal enemy, of Broussais, in another place remarks: "It seems, really, that an alteration which occurs so frequently, cannot be considered as accidental, and that those pathologists who have assigned so important a part to the stomach in the affection, which they call gastro-enteritis gravior, and which we call typhoid fever, had not committed so capital an error as it seems generally now to be conceded that they have. But researches made on a great number of bodies opened for some years past at the Clinique, show us the little importance of that alteration relatively to typhoid fever, for it is neither more frequent, nor more intense in this latter, than in most other diseases which cause death. Also, it comes on only during the last days of life, and cannot be considered as the lesion which provokes the development of the typhoid affection." "Lastly," he adds, "let us conclude from all these facts that the different pathological states of the stomach observed after death, either the injection, softening, slate color, or thickening of the mucous membrane in

persons who have died of the typhoid affection, do not constantly manifest themselves by any symptoms which can cause them to be known, and that it is impossible to say during life, with any certainty, whether we shall find in the stomach any appreciable alteration, or what will be its nature."

Such are the facts which we desired to lay before the profession, relating to the disputed points in question, together with the sentiments of the leading authorities of the present day, on this important subject. Of their value it is for it to judge, and decide accordingly. The profession is popularly charged with disagreement, and the pages which precede, bear witness to the truth of the sarcasm. But, difference of opinion is usually associated with difference of education. All do not keep pace alike with the progress of science, either from a disinclination to study, or a lack of opportunity; and hence, in consultation, it must often happen that one will refuse to acknowledge as the received doctrine, that of which he is ignorant, and continue to assert opinions which his better informed colleague knows to be erroneous. The old cling with pertinacity to the views which they imbibed in their youth; they reluctantly accept the information attempted to be imparted to them by younger men, and are prone to consider it in the light of a scholastic innovation, of which time will refute the fallacy and procure the oblivion. The young, on the other hand, seduced by the specious pleading of some favorite teacher, and captivated by the apparent truth of a particular theory, assert it with zeal, as if it were incontrovertible; are ignorant of, or indifferent to, the objections by which it may have been assailed; and look with contempt or pity on antiquated theories, and the promptings of age, when founded solely upon the results of observation and experience. Feelings, too, far from honorable to their authors, mingle with, or excite controversy; truth is thus obscured and perverted, error is engendered, dissension is established, and the profession brought into disgrace by the unworthiness of its votaries. Of these, the love of notoriety on the one hand, and envy and a fondness for detraction, on the other, appear to be the chief; but prejudiced or inaccurate observation, hastiness of generalization, and a readiness for theorizing upon insufficient foundations, must not be omitted in the list of the causes of medical discrepancy. The only way in which this equality of knowledge, for the necessity of which, among medical men, we contend, can be obtained, is, carefully to chronicle conflicting opinions, to estimate the value of recorded facts and proposed theories, and to deduce thence some conclusion which may be universally known, acknowledged, and adopted. Thus, and thus only, it seems to us, can unseemly differences and discussions be avoided, which excite personal animosity, lower the professional character in the public eye, and lead to a doubt both of the knowledge of the practitioner and the value of the art. What contest of opinion would of necessity not result, if there stood at the bedside of a typhous patient three physicians, one of whom should have been nurtured in the theories of Cullen or of Brown, the other a staunch supporter of the doctrines of Broussais, and the third a rigid essentialist? It is from the diffusion of knowledge by means of *periodical* literature, which many read who read nothing else, that much is to be hoped for toward the attainment of this most desirable object. And if the consideration of fever be, as we believe it is, that which, as a matter of scientific investigation, possesses the *highest* interest, the attempt we are now making to disseminate the views of its

nature, now most commonly entertained, and to establish its true character, cannot be without advantage. In medicine, as in law, it is indispensable that the jury be well informed on all sides of the question, before a verdict can be rendered; and when disputed points shall be made to undergo this judicial strictness of public investigation, the truth will oftenest appear, and a huge stride will be made toward the eradication of one of the greatest of the *opprobria medicorum*.

We come next, in our pyretological inquiries, to

PART II.—*Continued Fever in general and Typhus in particular.*

We hear continually in conversation, and we read in books, of typhus fever, typhoid fever, and of diseases presenting typhoid symptoms. What is the precise signification which is to be attached to each of these terms? *Typhoid* is evidently only the diminutive of typhus. Typhoid fever, then, must mean a fever less than typhus in the degree of its severity, though generally similar in its characters and nature, as varioloid is a mild form of variola—and not a distinct disease. Is then, what is called the typhoid fever, to say nothing now of its nature, a disease in which the symptoms present a less degree of intensity, and indicate a less severe degree of diseased action, than typhus? Generally, it is so; but it is not so always. The symptoms, then, of these two diseases are generally the same (varying always according to the local lesions which co-exist), except as to the degree of their intensity. Such, we believe, will appear to be the fact. The question which next arises is, whether there is any other difference. Waiving the consideration of this point for a moment, let us ask, what is the difference between typhus and typhoid *fevers*, used in the sense of milder degrees of the same disease, and other diseases in the course of which typhoid symptoms show themselves? The reply is, that in the latter, the typhoid symptoms supervene after a shorter or longer time, the disease having commenced with simple inflammatory symptoms; the criterion being, the order of the succession of the phenomena. But suppose it to be asked whether a case, in which, alone, the evidences of an acute cerebral or other inflammation should be discovered after death, every other viscus appearing healthy, the typhoid symptoms having appeared from the beginning, is one of typhoid inflammation, of typhus, or typhoid fever? For our own part, we should say that, unless there was anything instructive in the physiognomy of the case, and we do not well see why there should be, there is no difference between them. By the "physiognomy" of the case is meant that impression respecting its nature, as contrasted with others with which it might be confounded, which an experienced man receives from the appearance of the patient and the character of the symptoms, *i. e.*, the general aspect of the case, which he can neither convey to others nor describe to himself. It may be said that typhus fever in such a case is inflammation. Such is, in fact, just the ground assumed by Broussais; for to contend for the separate nature of typhus, is to essentialize the disease. The question would then be, is there, as Pinel says, essential typhus fever with inflammation, or as Broussais would have it, inflammation with typhoid symptoms? The case which we have proposed, is the intermediate of two others; the first, typhous fevers in which there are no inflammations; the last, typhous fevers in which the inflammatory symptoms supervene after a longer or shorter time. But what shall be said of a case of traumatic erysipelas, in which

the patient becomes typhoid and dies? *He* had not typhus, of which the erysipelas or phlebitis was a secondary lesion. And again of variola, measles, and scarlatina; how do they become typhoid? Is there something in the cause which induces the eruption, that induces the typhoid symptoms? or are they somehow developed in the progress of the malady? Absorption of pus, which might be adduced to explain the fact as regards variola and phlebitis, does not apply to measles, the œdematous form of erysipelas, etc. Again, why do we not see typhoid symptoms accompany phthisis, in which Peyer's plates and the glands of Brunner are extensively ulcerated, or in the cholera infantum? Is there, then, *a something* which causes typhoid symptoms independently of inflammation? and if so, what is it? How do the inflammations become typhoid? Let us add, whence is the true typhus derived which results, in animals, from the injection into their veins of poisonous substances, or in the human species, from the use of putrid meat, a wound received in dissection or in skinning an over-driven ox, or the contagion of the glanders? In a word, what is the nature and the cause of typhus and typhoid fevers, the terms being used synonymously, as expressing only a difference of degree in the severity of the symptoms?

We have now before us two questions for consideration. The one has just been stated; the other is—Is there any other difference between typhus and typhoid fever, except what consists in the greater severity of the one than of the other?

In answer to the last of these inquiries, it is first to be established that there may be in cases of typhoid fever, used in the sense of ulceration of the intestines, or dothinenterite, symptoms of malignity equal in degree to those which can present themselves in cases of typhus occurring without this lesion. Of this any one may convince himself by an examination of the cases of fever attended with ulcerations of the bowels, reported by Tweedie and Southwood Smith; and similar instances might be selected from the writings of Andral, Chomel, Bouillaud, and Louis. Admitting, then, as we believe may be done, that such cases do occur, differing only in the fact of the peculiar anatomical lesion, the term *typhoid*, in the sense of a *diminution* of severity, is no longer applicable to all. Such cases, *if there be no other differences*, are cases of typhus. Before we proceed to consider this point, let us make a few quotations. "Is the typhus of camps, jails, &c.," asks Chomel, "the same as the typhoid affection? On this subject we have only the most vague and uncertain data, and it may be said that, on this point, medicine remains where it was twenty years ago. The most recent documents which we have on this subject, are too contradictory to give any fixed idea of the nature of the *lesion of the intestine* in typhus. Thus, notwithstanding all that has been said on the typhoid affection for some years past, its identity with typhus, *though probable*, is not yet certain." There is, in this passage, no contest as to the identity of the symptoms in the two diseases. Again, he says, "that he prefers to others, as a designation for the diseases described by authors under the names of severe continued fevers, be the form in which they show themselves what it may—inflammatory, bilious, mucous, adynamic, ataxic, slow, nervous, or other, all of which are only *varieties of the same disease*, the term 'typhoid' affection, or fever, *owing to the analogy which it offers in its symptoms* to the typhus of camps." The fevers enumerated above are, then, we perceive, only varieties of the same disease. They are all typhus fevers, varying in

form, according to local circumstances. Let us particularly note this quotation from Bouillaud: "*There are cases in which typhous phenomena appear in persons attacked with fever, without, therefore, Peyer's plates being inflamed,* and then, the local symptoms, the idiopathic functional disturbances which characterize this phlegmasia, fail." This Louis calls "simulated typhoid affection." Now, what are these but cases of typhus? Christison, in speaking of dothinenterite, says, that "its symptomatic fever is peculiarly typhoid, *and undistinguishable by any essential character from the true primary typhus of British pathologists,*" in which, according to him, this ulcerated state of the intestinal glands is rarely met with. Andral observes, that "the disturbance which gives rise to the phenomena called adynamic or ataxic, or in other words, to the typhoid state, may be the result of several lesions, different in their nature and their seat." And again: "thus the ataxo-adynamic, or typhoid state, may be developed in consequence of a great number of affections widely differing from each other; it is a collection of symptoms identical as to their ultimate seat, but not as to their origin. Thus, symptoms, identical also as to their nature, differ only in their greater or less intensity, progress, and duration, according to the progress and duration of the affections, in *consequence of which* (?) the typhoid symptoms have been developed.* This is, we think, *all the difference* [a mere difference of intensity] between a typhoid fever which is developed in connection with phlebitis, for instance, and that which follows a dothinenterite." Again, speaking of inflammatory diseases which become typhoid, he says: "In all these cases we shall still find the different symptoms which the cases under the first article (typhoid fevers) presented us, and the aggregate of which constitutes the ataxic and adynamic fevers, such as Pinel has described them." Speaking of typhoid erysipelas, phlegmon, diseases of the urinary passages, hepatitis, variola, phlebitis, metritis, &c., he says: "these are not the typhoid fever of Chomel and Louis, but still the symptoms of this fever differently combined." And again: "In a word, there is scarcely any disease having its primary seat either in the solids or in the blood, in consequence of which we may not see typhoid fever sometimes developed, and much more frequently, simple typhoid phenomena, or under other forms, the different symptoms to which Pinel has given the names of ataxic or adynamic."

From the remarks which have preceded, we may safely, we think, lay it down as a fact, that typhus fever, of whatever kind, and with whatever lesions co-existing, and also when existing without appreciable morbid lesions, is, so far as symptoms are concerned, the same disease as the typhoid fever of Chomel and Louis; differing only as to the intensity of those symptoms, and in some particulars, as to duration, progress, &c., according to the particular lesions with which individual cases are attended. 2dly, That inflammatory diseases which become typhoid, can in no wise be distinguished from true typhus, including the "typhoid affection," except by the circumstances of their earlier stages. 3dly, That cases of local inflammation in which the typhoid symptoms exist from the very first period of their invasion, are only cases of typhus, with a local lesion; and, 4thly, That every ataxic or adynamic fever is but typhus; the symptoms being only differently combined, and the progress and dura-

* This expression is incorrect, as will hereafter appear—it should be "in connection with which," &c.

tion varying according to the particular symptoms attendant on each; in other words, according to the local lesions which accompany them. We perfectly concur, then, with Andral in saying that "we must not retain the term typhoid fever," unless it shall hereafter appear that a particular affection deserves the name: and we further agree with him in saying that "we admit as a possible, but never as a necessary consequence, a typhoid state in a great number of diseases; that is to say, a state in which there appear some general symptoms, more or less similar to those which characterize typhus; evidences that the blood and nervous centres now participate in the disease."

Dothinenteritis.—Is it a disease sui generis? We have just observed, that unless it should hereafter appear that a particular affection should deserve the special name of *typhoid fever or affection*, that term should not be retained in nosology. The affection here alluded to is *Dothinenteritis*, or an inflammation of the muciparous glands (patches, agminated and isolated) of the small intestine, and chiefly of the ileum, in the progress of which these organs (Peyer's plates and Brunner's glands), enlarge, ulcerate, and slough; and, extending their ravages laterally to the intervening mucous membrane, come at length to form some of the vast ulcerations which constitute the most remarkable lesions met with in the autopsy of fevers. This name was affixed to the disease by Bretonneau, of Tours, who first, in any remarkable manner, called the attention of the profession to the subject in an elaborate treatise; although in the able work of Billard, so prematurely lost to the science which he adorned, on the Gast. Int. Muc. Memb., and which preceded that of Bretonneau, will be found a full account of these morbid appearances, and a distinct allusion to their connection with typhoid fever. We proceed to inquire whether this lesion, of which we readily admit the great frequency, is merely a complication, an occasional, incidental, and secondary affection, occurring during typhus fever, *or whether it be a disease per se*, and wholly different from that to which the name of typhus has long, in all countries, been appropriated. A typhoid inflammation merely, or one or other of these two, it must be; and in the latter event only, and then scarcely with propriety (since, as will hereafter appear, it is often identical as to its symptoms with typhus), can it be deserving of the separate generic appellation by which, of late, it has been designated.

In proceeding to the investigation of this question, let us first inquire whether, in all fevers characterized by typhoid symptoms, *this lesion is constant?* Louis and Cruveilhier affirm that it is; Chomel, Bouillaud, and Andral, all of whom describe the fever of France, deny that it is so. Dr. Gerhard, of Philadelphia, insists on the difference between the two diseases *in toto*. Christison and Tweedie, as also Roupell, dissent wholly from Louis and Cruveilhier, as to its constancy in the fevers of Britain. Before citing the first of these last named authors, we wish the reader particularly to remark that the description he has given of what he afterwards designates "the typhoid affection," applies, according to the title-page of his work, to "the disease known by the names of Gastro-enteritis, putrid or adynamic, ataxic or typhoid fever," and, of course, includes what is generally termed "typhus," so that as to symptoms, &c., he does not make any distinction between them, and the dothinenteritis, according to him, is not a separate disease, but is common to them all. In fact, he speaks, on the 119th page (Translation), of a case presenting the worst symptoms of typhus fever. "As," he says, "in all the

cases where the patients died between the eighth and twelfth days, the first symptoms were connected with a lesion of the intestinal canal, we must conclude that the time at which the alteration of the elliptical plates of the ileum commenced, is the same as that of the disease, and we must not consider the lesion as one of the effects of the fever, *but that it forms the anatomical feature of it.*" He continues, "of all these lesions, one only was *constantly found*, viz., alteration of the elliptical plates of the ileum, to which may be added, a morbid change in the mesenteric glands. I have considered it as *inseparable from the disease we are now studying, and as absolutely forming its anatomical character.*" "I must conclude," he adds, "that the lesion of the elliptical plates began at the commencement of the disease, and that all the other lesions must be considered as merely accessory, or secondary." Again, "more or less severe lesions of the elliptical plates of the small intestine, were always found at the autopsy, which lesions were more serious according to the proximity of the patches to the ileo-cæcal valve, presenting remarkable differences according to the duration of the disease, and accompanied by analogous changes of the corresponding mesenteric glands. The other organs were frequently found diseased, but these lesions were not constant, and differed in some respects only from those observed in others who died of other acute diseases." Cruveilhier states that he found the abdominal complications so frequent in fevers, as to induce him to propose the term "intestinal," and asserts that "the follicular disease occurs under three forms: adynamic, inflammatory, and ataxic. Inflammatory complications may exist and give a particular character to the disease, but still the original primitive follicular disease exists and constitutes the most characteristic feature of the malady, which is *always* seated in the extremity of the small intestine, and chiefly about the ileo-cæcal valve."

Dr. Gerhard, of Philadelphia, says, that in fifty autopsies of typhus, there was but one, and in that one the diagnosis was doubtful, in which there existed the slightest deviation from the natural appearances of the glands of Peyer. In all the cases, these glands were remarkably healthy. "Thus," he says, "the triple lesion of the glands of Peyer, mesenteric glands, and spleen, constituting the anatomical character of the dothinenterite or typhoid affection, did not exist in the epidemic typhus of Philadelphia, in 1836. The two diseases are no more identical than pneumonia and pleurisy. The anatomical characters of these varieties of fever, continued, remittent, etc., are peculiar to themselves, and it is as impossible to substitute the lesion of the small intestines observed in dothinenterite, for the pathological appearances of typhus, as it is by treatment, or other means, to transform the eruption of measles into that of small-pox." He also establishes certain diagnostic differences between the diseases, into the accuracy of which, however, we shall not in this place inquire. We come next to the work of Chomel on the typhoid fever. "The anatomical lesions," he says, "which are observed in patients who die of the typhoid malady, are numerous and distinct, and may be referred to two very different groups. The first comprises those which are met with *almost constantly*" (the modification is deserving of note) "in this affection, and do not exist in any other. (?) They occupy particularly the intestinal follicles and mesenteric ganglia. The second group comprises the accidental lesions, such as inflammation of the mucous membrane and some parenchymatous organs; they do not belong to typhoid fever, for they are not found in all who die of it, and are met with

every day in those who do not offer the symptoms of that malady. Thus, no observant physician, no man versed in the study of pathological anatomy, now-a-days, regards this malady as a gastro-enteritis, nor a gastritis." This is a reflection both upon Broussais and Bouillaud. "It is now as clear as the day, that it is not in the mucous membrane of the stomach or intestines, that the peculiar anatomical lesion resides, which is characteristic of the affection, but in the intestinal follicles, where it shows itself in forms which vary according to the period of the disease, but which, in each of its periods, has a special form and cannot be confounded with any other lesion. This fact, too evident to be contested, has induced several partisans of the doctrine called physiological, to designate this affection, no longer under the name of gastritis, nor that of gastro-enteritis, but under that of follicular enteritis. This was a mode of preserving something of the primary denomination, while they acknowledged that the disease, which is still called enteritis, is not an inflammation of the intestine itself, but only of the follicles which are scattered over it." On this over-nice distinction, we shall hereafter have something to say. "Follicular inflammation," continues M. Chomel, "is so frequent, that in four years not a single exception was met with. But Louis and Andral show cases in which during life *the symptoms had been exactly those which are observed in the typhoid malady, yet in which were none of the alterations proper to this malady*, nor did any exist to which the symptoms that appeared could be referred." Andral observes: "But in some of these cases only, we shall see these fevers present the same course, the same duration, *the same connection of marked phenomena* as those which are connected with the special inflammation of the follicles. They will be typhoid fevers, in the sense which M. Louis has attached to the term. They are, no doubt, rare exceptions, and it remains no less true that the typhoid fevers, such as M. Louis has described them, coincide *almost always* with an affection of Peyer's glands; but these exceptions, of which Louis has himself published cases, it is important to notice. "We see from this quotation, that the similarity of the symptoms of typhus fevers with those of dothinenterite is asserted, in cases in which, however, the latter did not exist; and to make the case still stronger, Andral says of one of his cases, "all the symptoms observed in this individual are those observed in persons suffering from dothinenteritis, but no trace of an affection of the mucous follicles existed. Nothing but an erythema of different portions of the gastro-intestinal mucous membrane." Of this, we have ourselves seen a precisely similar example. Andral continues: "In persons who die with *one or other* of the morbid groups called *essential fevers*, some present no appreciable lesions of the small intestines; in others, we find lesions similar to those met with in other acute diseases. But, in the greater number, the small intestine presents a peculiar lesion, which in its acute form is almost exclusively confined to what are called essential fevers." Here again we observe that the follicular enteritis is ranked as an essential fever, and that all essential fevers are regarded as generally identical. "Follicular enteritis then," he says lastly, "appears to us to be the commencement of a very great number of what are called essential fevers, and *particularly* of that which Chomel and Louis called typhoid fever. *This latter affection may, however, present itself in cases where the intestinal follicles are not diseased.*" We have already, by the quotation from the title-page of Louis's work, pointed out that he considers his typhoid affection, and the other forms

of ataxic or adynamic fevers (typhus), as identical and synonymous. In fairness, we ought to introduce here a citation from Chomel, which we find we have omitted. "But," says he, "after having lessened the importance of the follicular lesion in typhoid fever, we must repeat how great is its value as a characteristic lesion of the disease, and also repeat, *that if not strictly constant*, it is extremely rare to find it wholly wanting, and that there exists not a single authentic instance of this lesion, in a subject who had *not* offered the symptoms of typhoid fever." This assertion we shall presently show to be erroneous. We come now to the British authorities. Dr. Stokes (Dublin Journal Medical Sciences, March, 1839) observes: "I consider typhus fever as *an essential disease*, not symptomatic of any known local lesion." "There can be no doubt that the typhus of Great Britain and Ireland, is a disease of the whole system, *not symptomatic of any particular* local lesion, &c., capable of destroying life *with* various lesions, or without any appreciable change in the solids." "With respect to the local lesions, I consider them as much secondary to the general disease, as the pustule in small pox is to the disease variola. Their not unfrequent absence in the worst cases of the disease, proves that *they are not the cause of typhus*, while in cases where they do occur, we observe a signal want of proportion between their amount and the severity of the symptoms. They are, *in the fullest sense, inconstant in their seat and extent, incompetent to the explanation of the symptoms*, and unnecessary to the characteristic phenomena of the disease." He adds that there undoubtedly exists a much greater disposition to intestinal ulcerations in the fevers of Paris and other situations on the continent; but that had Broussais examined the typhus of Great Britain and Ireland, he would never have formed his theory of fever. Respecting the opinion of Dr. Lombard, of Geneva, that the typhus fever of Ireland is a peculiar affection, differing from that of the continent in the absence of ulcerations, (an opinion in which Louis is disposed to participate,) he says: "the fact is, that intestinal ulcerations have been repeatedly observed in the typhus of Ireland, their amount and frequency varying with the epidemic influence." In the epidemic of 1826 and '27, "the whole group of vital and cadaveric phenomena, corresponded almost exactly with the dothinenteritic typhus of the French authors." "Since then, no severe or decided epidemic of fever has occurred; but cases of typhus are to be met with, with *or without* this peculiar lesion. The researches of Bright and others, in London, and of Staberoh in Glasgow, show the same. And "we cannot then found any general distinction of British and continental fevers on this circumstance; *the difference is in the degree of liability.*" Dr. Stokes's first case is headed, "Petechial typhus fever, *with extreme prostration*—Death. *Absence of organic lesion* (of any kind, in any part of the body)." Similar views are promulgated by Dr. Graves, of Dublin, in his System of Clinical Medicine, recently published. Christison, whom we have previously quoted as saying that the typhoid affection of Louis is undistinguishable, by any essential character, from the true primary typhus of British pathologists, states that they and "practitioners extensively conversant with the features of typhus, invariably consider that enlargement of the glands of Peyer, is a rare, and in many parts of Britain, a very rare occurrence. In the infirmary of Edinburgh, it has been constantly and diligently looked for during the last sixteen years, in several epidemics, yet it is found only often enough to make the pathologist acquainted with its phenomena, and

keep him in mind of its existence." "It seems reasonable to conclude," continues Dr. C., "that the signs of structural derangement, ascribed by M. Louis to typhoid fever generally, are most of them peculiar pathological states, secondary, not to fever in general, but to one of its secondary disorders, dothinenterite." "We have traced," says Dr. Tweedie, "the progress of typhus fever to its fatal termination, and on the most minute examination, have been unable to discover the follicular disease which Louis and Cruveilhier state to be so constantly its cause; while, per contra, we have met with follicular enteritis in individuals whose symptoms had no resemblance to those of typhus fever." This statement will be observed to contradict the assertion recently quoted from Chomel. And here it is particularly to be noted, that none of these authors, in objecting to the views of Louis, have attempted to palliate the discrepancy by any reference to a difference of symptoms, these having appeared to them in all cases the same. The only person who has attempted such a distinction as a ground of diagnosis, is Dr. Gerhard, of Philadelphia, whose authority, great as it is, cannot be considered as outweighing that of Christison, Tweedie, Chomel, Andral, and even that of Louis himself (who tells us that one of his cases presented the very worst symptoms of typhus), who all acknowledge a perfect identity in the symptoms of essential fevers, with whatever lesions they may be complicated. Upon this point we have the express authority of Chomel, who says, that "we are to conclude from the facts related, that the varieties of the anatomical lesion of the follicles and ganglia, *do not disclose themselves to us by any particular phenomena*, and that all the symptoms, except perhaps the diarrhœa, the abdominal pain, and the gurgling, are the expression of the influence of the disease upon the entire economy, of the disorders which it induces in the principal functions, and belong rather to the disease itself, than are the effects of the lesions of the follicles. It would be easy to show that each of the symptoms particularized as diagnostic by Chomel, is wanting in certain cases. Dr. Gerhard's emphatic assertion, that typhus and the typhoid fever of Louis are no more identical than pneumonia and pleurisy, cannot then so easily be acknowledged to be correct, and we are not a little surprised that so able a physician should say, after a detail of fifty cases of fever, in which *no* alteration of Peyer's glands was found, that the fever *constantly* met with in the United States is identical with that of Paris, but not of London. If intestinal ulceration necessarily constitutes the "typhoid affection," then many cases of that disease are met with in Great Britain. There is, then, no generic "fever of London" with which a generic "fever of Paris" is to be contrasted. But of this anon.

Dr. Roupell, in his very able and useful, though concise treatise, asserts his conviction of the identity of the disease, called by Louis the "typhoid fever," with that which Hildenbrand has designated as typhus. We find in it, he says, all the leading indications. This "typhus" of Hildenbrand, he considers, be it observed, as identical with the typhus fevers he was in the habit of seeing as epidemic in London. And he further adds, in speaking of the fever described by Louis and Chomel, and that of Hildenbrand and his own observation—"the impression on my mind, arising from the correspondence between the leading features, as already detailed, is, *that the two fevers are identical.*" "M. Chomel," he says, "makes two varieties, typhus and the typhoid malady: the latter, he says, lasts longer than typhus, and the rash appears on the

fourth, and in the other, not until the eighth day. These, surely, *are the same disease*, varied only by circumstances:" In the article "Fever," in the Dict. de Medecine, Littré thus expresses himself: "I have said that often fevers produce their terrible effects, with a very slight lesion: I might go further, and say that the lesion itself is sometimes wanting, while, nevertheless, the symptoms preserve at once their gravity and their special physiognomy. It is an established fact that there are typhoid fevers *without intestinal lesion*, or with one that is very slight; we must, then, admit that the nature and intensity of fever *does not reside in the local lesion:* that this lesion is an important and characteristic manifestation when it exists, but *it is not essential.*" "An opinion," says Dr. Alison, certainly one of the most philosophical writers of the day, "has lately become rather prevalent, that there are two kinds of fever, to which the names of typhus and of typhoid fever have been given, of which the first is usually unconnected with any affection of the mucous membrane, is attended with the eruption, and is more contagious; the latter is necessarily connected with inflammation going to ulceration of the mucous membrane, *is unattended with eruption*, is more protracted and has *little contagious property*. This term, typhoid fever, is to be regarded as synonymous with *gastro-enterite* and with dothinenterite. If it be understood that the intestinal inflammation in this last case is of *specific* character, this doctrine can have no injurious practical results; but it is certain that some cases resulting from the contagion of the usual spotted typhus show all the symptoms and post-mortem appearances attributed to the typhoid fever; and, therefore, it seems most probable that the differences observed are only varieties depending on constitution and on the agency of other causes affecting the constitution, besides the exciting cause of the disease." The first two lines of this extract, which we have placed in italics, respecting the absence of eruption in, and slight contagiousness of, typhoid fever, are, with deference to the accomplished author, inaccurate; the former entirely incorrect. (p. 446.) In another place (p. 403, Outlines of Path. and Practice of Med.) we find him stating, "that all the continued fevers of this climate must be regarded as fundamentally the same disease." Again, he asserts, p. 406, "the occasional entire absence, after fatal fever, of any appearances which can strictly be called morbid; a rare case, but admitted by the most accurate morbid anatomists as an unequivocal result of their inquiries."

The last author whom we shall cite, is Cruveilhier. "We cannot," he says, in his splendid work on Pathology, "without making the sacrifice of the seat of the disease, and of the lesion which essentially constitutes it, preserve the denominations, inflammatory fever or non-putrid synochus, adynamic, ataxic, *typhoid* fever, *to designate follicular enteritis*, for there are inflammatory, adynamic and ataxic fevers which are pneumonic, pleuritic," &c. Elsewhere he acknowledges that "if the typhoid form is common to all the painless inflammations, it may be said that *none presents it in a higher degree than acute follicular enteritis.*" He then adds, that "the ataxic form is not less frequent than the typhoid," and continues: "How can we refer symptoms so serious as those which we have now enumerated to a lesion of the intestinal canal so circumscribed. Is it not more probable that these symptoms depend upon a cause acting on the entire of the economy. This local malady (dothinenterite) is perhaps only an eruption analogous to the labial or cutaneous

eruptions, a sort of crisis, which, salutary if it had been carried to the skin, becomes fatal because it is directed upon an organ essential to life. In the eruptive diseases, certainly, it is not the eruption which governs the fever, but the fever which governs the eruption: it would be absurd to say that the morbillous or variolous fever is symptomatic of an eruption which does not yet exist! These remarks apply exactly to acute follicular enteritis. The fever precedes the inflammation twenty-four or forty-eight hours, or even three days. The fever persists and augments all the time that the local mischief increases, which is *not the cause of the fever*, but which is its measure." "As soon," he adds, in another part of his essay, "as it was established that the fevers called putrid or adynamic, malignant or ataxic, almost always left after them the lesion (follicular enteritis) which we have described, this question necessarily arose: Is this lesion the point of departure of all the symptoms? is it, in a word, a crisis? does it govern the disease, or is it governed by it?" The answer to this query, though not directly expressed, may be gleaned from the passage previously quoted. To the same effect, we find the learned Copland, in his Dictionary, asserting that "instead of receiving dothinenteritis as intimately connected with the nature of typhus fevers, I consider it only as one of several changes superinduced in the progress of the disease, but one of the most constant and important; its symptoms inseparable from the typhoid states of fever and the expression of the disease on the whole economy, rather than on this part of it in particular." (Part. III., p. 913, par. 91, A. Fever, Path. of.)

We think it must be allowed to follow from what has now been said, that the lesion of Peyer's glands is, though very frequent, and evidently much more so in the fevers of France and of our own country, than of Great Britain, *not* constant in what is called the typhoid fever, or affection; that when present it does not always so modify the disease as to render it distinct, sometimes even from the very worst forms of typhus, nor to enable us to distinguish them always by their outward appearances, and consequently that it cannot be considered as a distinct disease. If so, it can only be viewed as one of the many forms of organic alteration which are met with in fever generally; whether as a primary, or secondary lesion, we shall presently inquire. To similar conclusions we find that Dr. Hale has come, in his Remarks on the Pathology of the Typhoid Fever of New England. (Boston, 1839.) While, with Dr. Jackson, he asserts that in every case the lesions in this fever are identical with those described by Louis, he does not think that in the present state of our knowledge, at least, the fever called typhoid or dothinenterite, can always be distinguished, during life, from that designated typhus. And in the Review from which we copy this statement, it is said that Dr. Lombard, of Geneva, and other observers, who were familiar with the typhoid fever of Paris, were much astonished at finding in England cases which appeared to them genuine typhoid fever, but which showed none of the pathological appearances they had so confidently expected: so much so, that Dr. Lombard, under the first impulse, abandoned the opinion formed upon six years' investigation, and concluded that typhus fever was a general disease affecting the whole constitution, rather than a malady depending on any local inflammation or change of structure. And Dr. Hale himself thinks, that whereas the lesion is many times so slight as not to explain the phenomena and fatal result, it is rational to consider it as an effect, like the general disturbance of the system, of some cause

at present unknown. (Am. Journ. Med. Sc., 1840.) The wisdom of Andral's conclusions on this, as on every subject of which he treats, will appear from the following extract: "All cases of follicular enteritis are not accompanied by stupor, or the different phenomena of typhus (ataxic or adynamic) fever: all do not resemble typhus; they are sometimes merely inflammatory, bilious, or mucous fevers; to use, for a moment, the language of Pinel, and hence, although we may, no doubt, as M. Louis has done, reserve the term typhoid fever, or affection, for the morbid state which proceeds along with the affection of Peyer's glands, such a distinction is nevertheless purely arbitrary."

We have now endeavored to show that there exists between typhus and the typhoid fever, or affection, or dothinenteritis, no essential difference in symptoms; that all ataxo-adynamic fevers are either typhus or typhoid, as we may please to designate them, which is equally the view taken by all the writers we have cited; that dothinenteritis is not a constant lesion; that it is attended with other co-existing lesions, and that they may exist without it; that, to use the words of Andral, the small intestine may present *no sort of marked lesion* after different kinds of fever; and that all the shades of vascular injection which have been met with in the lesions of the intestinal mucous membrane, are met with in the diseases in question, and are sometimes, though rarely, alone observable.

We are now prepared to pass to the second part of our inquiry, and ask, what is the nature and the cause of fever?

(*To be continued in our next.*)

ART. IV.—*Remarks on a peculiar form of Paralysis.* By WILLIAM P. BUEL, M. D., of New York,

I HAVE met, principally in dispensary practice, with a form of Paralysis I do not recollect to have seen described in books. It affects the nerves and muscles of the fore arm, the hand, the thumb, and the fingers; producing loss of muscular power, and loss of sensation, partial or complete, from the bend of the elbow to the tips of the fingers. From the fact that when the arm is extended and leaned in the direction of pronation, the hand drops powerlessly down, with an utter inability to extend it, the affection has been called the "drop hand."

The muscles and integuments of the fore arm, hand, and fingers, receive their supply of nervous influence from two nerves—the radial and the ulnar. The radial nerve, after emerging from the bend of the arm, gives off branches to the pronator teres, flexor radialis, palmaris longus, to the flexors of the thumb and fingers, and to the pronator quadratus. Passing with the tendons of the flexor muscles under the ligament of the wrist, it gives off five branches, which are distributed to the adductor muscles of the thumb, and to the radial side of each of the fingers.

The ulnar nerve, after passing the condyle of the humerus, goes down in connection with the ulnar artery to the wrist, where it divides into two

branches; one of which, the main branch, passes into the palm of the hand, while the other getting on to the back of the hand, branches over the tendons, and is finally distributed to the back of the little and ring fingers. The other branch, passing under the palmar aponeurosis, is distributed on the ulnar edge of the hand, the outer edge of the little finger, and the sides of the little and ring fingers.

This brief account of the nervous distribution will enable us better to understand the phenomena which occur in the subjects of this affection.

The motions of the fore arm upon the arm, depending entirely upon muscles seated above the elbow, are not impaired. The muscles which move the hand, the fingers, and the thumb, and which communicate the various motions of flexion and extension, pronation and supination, abduction and adduction, are paralysed, in some cases partially; in other cases, the paralysis is complete. The hand, as already observed, drops helplessly when the fore arm is extended and turned in the direction of pronation. The power of grasping between the thumb and fingers is, in many cases, entirely lost; as also that of closing the fingers upon the palm. As in other cases of paralysis, the flexors predominate over the antagonist muscles of extension, and the fingers are in a state of some flexion.

The loss of sensation, somewhat enfeebled at the upper part of the fore arm, is increasingly so as we pass downwards, until we reach the fingers, where it is often complete. The ulnar nerve from its being exposed at the point where it passes round the condyle of the humerus, being more subject to the cause of the affection than the radial, those parts of the fore arm and hand receiving their nervous supply from it, are generally most affected, sometimes exclusively so. One branch of the ulnar nerve, as already mentioned, being distributed over the back of the hand, to its ulnar edge, and to the little and ring fingers, the loss of sensation in these parts is complete, being numb and powerless; while the middle and forefingers and the thumb, retain the power of motion and sensation.

The cause of the affection appears, in all the cases I have met with, to be one and the same; viz., the long continued pressure of the weight of the body upon the nerves of the fore arm in sleep. The subjects of these affections tell you they went to bed at night as well as usual; and when they awoke in the morning, they found the fore arm and hand numb and powerless in the manner already described. They had no doubt gone to sleep with the fore arm under the body, and remaining in that condition for several hours, paralysis, the usual consequence of continued pressure upon nerves, is the result. Those cases I have met with, occurred in hard-working people, whose slumbers after the labors of the day are dead and heavy. In several instances, I suspected that some extra narcotism had been produced by libations of beer and spirits.

Treatment. When I first met with these cases, I treated them by stimulating frictions along the track of the nerves, and repeated blisters on the palmar surface of the fore-arm. This treatment was entirely unsuccessful. I then thought of applying moxas, and this method was attended with complete success. The first case I met was treated with stimulating liniments and blisters for two or three weeks, without the slightest improvement, the hand and fingers remaining perfectly powerless. I then applied a single moxa on the facial surface of the fore arm, and in a week the improvement was obvious. I was obliged, however,

to apply a second or third moxa, before the cure was complete. Since that time I have treated four or five cases, applying in every instance from two to three moxas up and down the palmar surface of the fore arm; and the cure is generally completed in from three to four weeks.

Finding this method so successful, I have tried no other. I have been told that in the New York Hospital, electro-puncture has been used successfully in this affection; but I have not had the opportunity of trying it.

The form of moxa which I have used, is that of small bits of gum camphor. This form, as it is always at hand, is rapid in its application, and produces as little pain as any method of applying the actual cautery to the human flesh, I prefer, as well in this case, as in others where the remedy is advisable.

[From the striking analogy of the cases above detailed to certain symptoms resulting from exposure to some of the preparations of lead, we addressed a note to Dr. Buel, suggesting a further inquiry; and to this, the following reply was made. EDITOR.]

Dear Doctor,—In the note you were kind enough to send me this morning, you remark, "there is a peculiar affection, arising from exposure to the effects of lead, called by the workmen the *wrist drop*, which bears a marked resemblance to the 'peculiar form of paralysis' that you describe." "Some years ago," you continue, "cases of obstinate constipation of the bowels, accompanied occasionally by paralysis of the hand and fore arm, occurred so frequently among the soldiers of the United States army, from the use of white lead in cleaning their belts and gloves, that it became necessary to interdict its further use by a general order, which, at the same time, substituted pipe clay." "I have thought it," you add, "worth while to suggest the inquiry, whether the cases you refer to might not have a similar origin."

Thanking you for your kindness in making the suggestion, I would observe in reply, that when I first met with the cases described in my communication, I was myself forcibly struck with their analogy with certain affections produced by exposure to the effects of lead; and immediately directed my inquiries to ascertain whether they had not the same origin. I could not, however, ascertain that there had been in any such instance, any exposure to the effects of lead. Aware of the fact that beer and cider drawn through leaden pipes, have sometimes produced the peculiar symptoms following the introduction of lead into the system, I was suspicious that these cases might have been thus produced. Further inquiry, however, did not confirm my suspicions, but satisfied me that they were unfounded.

The affection I have described, differs from that which you refer to, in one very important particular. "Some years ago," you remark, "cases of *obstinate constipation of the bowels, accompanied occasionally by paralysis of the hand and fore arm*, occurred," &c. In these cases, and in all similar ones, arising from lead, the constipation of the bowels is the marked and prominent symptom to which the attention of the practitioner would primarily be directed. The cases I have described, were unattended with constipation, or indeed with any other disturbance in the general functions of the system. The patients, I believe, in every instance, declared themselves to be in their usual health, with the exception of the paralysis of the hand and fore arm.

The sudden access of the paralysis in my cases, differs from what might be expected from the gradual introduction of lead into the system in the manner you mention. In these cases, in every instance, the patients retired to rest in their usual health; they waked with the fore-arm and hand paralysed. I think then it is fairly to be inferred, that these cases were not the effect of lead.

1st. Because, on diligent inquiry, I could not ascertain that there had been any exposure to lead in any shape or form.

2dly. Because the affection was unaccompanied by constipation of the bowels.

3dly. Because the attack was sudden, and not gradual, as we should expect from the gradual introduction of lead into the system.

4thly. Because in all the instances, the affection was removed by the application of a remedy purely local; a result certainly not to be expected, supposing it to have been produced by a cause operating on the whole system, as the poison of lead.

Since my communication was sent in, I have seen two other cases. They differed in no respect from the cases therein described. Both were treated by the application of camphor moxas to the palmar surface of the fore-arm. The first was a strong, healthy Irishman, perfectly well in all other respects. At the end of a week from the application of the moxas, he returned without much improvement, and rather vexed at finding that his arm was no better than before, and that he had got in addition three sores upon it. At the end of the second week, the improvement was decided; and in another week the restoration was complete.

The second case was a woman of very intemperate habits. She returned at the end of the week with, as usual, but little improvement in that space of time. I have not seen her since; but I am confident, from the result in other cases, that she is cured.

Very truly yours, W. P. Buel.

Samuel Forry, M. D.

Art. V.—*Further Thoughts on the Hypotheses of the School of Chemico-Physiology.* By Charles Caldwell, M.D., Professor of the Institutes of Medicine and Medical Jurisprudence, Louisville, Kentucky.

No. I.*

When my Critique on the "Animal Chemistry" of Professor Liebig was closed, I did not foresee that I should deem it my duty, so far to return to the same subject, as to offer any additional remarks on the writings of

[* In view of the general tone of this article, we should perhaps hesitate in laying it before our readers, were it written by any other person; but as the distinguished author, in his "*Physiology vindicated, in a Critique on Liebig's Animal Chemistry,*" noticed in our first number, has already combated most successfully many of the errors of Liebig, we are disposed to allow him this privilege, regarding him as one speaking by authority. It shall always be our endeavor to maintain our pages free from all personalities in the discussion of scientific subjects; and in the particular case of Professor Liebig, whose remoteness forbids the idea of his defending himself, the claim is the more imperative. We are an admirer of Professor Liebig's writings, save his physiology and pathology.—Editor.]

that author. Nor, as relates immediately to the volume referred to, is it my intention to do so. A measure to that effect might well be accounted superfluous at least, if not illiberal and unmanly. It would too much resemble the ungenerous act of mercilessly treading on a fallen opponent—a step repulsive to manhood and magnanimity.

As respects the vaunted but crude pretension of "Animal Chemistry" to create a "new and improved era in physiology," or to advance, to the slightest extent, the condition and interest of that science, in its true character, I have fearlessly predicted its failure and overthrow. And already is that prediction (visionary and hazardous as many persons pronounced it), if not *actually*, yet *virtually* fulfilled.

I verily believe that there is not now, on this side of the Atlantic, a single physiologist of high standing, or at all *respectable* in his standing, and thoroughly acquainted with the notions maintained in Animal Chemistry, who will assert that physiology has received from them a shadow of improvement. I mean physiology in the true and accepted definition of the term—the science of living organized matter, including the principles and causes of its organization and action.

In the United States at least, if not, in like manner, in other portions of the world, FIRST IMPRESSIONS formed the lofty and wide-spread, but mushroom reputation of Animal Chemistry; and while its novelties were fresh, and but slightly, if at all, tested by reason and analysis, or by any other sort of competent ordeal, the same impressions continued to support it. But, by the influence of "sober second thoughts," hastily-formed and crude partialities have been dissipated; and the reputation erected on them, already shaken to its foundation, is destined to be shortly reduced to a ruin. So strong is my confidence in the fulfilment of this prediction, that I venture further to predict, that, a few years hence, when truth on the subject shall have been more fully unfolded, should the towering reputation which Animal Chemistry has enjoyed be at all remembered, the fact will be regarded as an object of wonder, reflecting but little credit on the physiological judgment and discrimination of the present day; and assuredly so on the judgment of those who have been most zealous in their encomiums on the work, and loudest in their hosannas to it.

Let it be distinctly understood, that I do not claim as exclusively my own, the death-blow inflicted on the standing and influence of Animal Chemistry. Far from it. On the minds of the few who have listened to my lectures, and read with attention my Critique on the subject, it is not, perhaps, presumptuous in me to believe that my labors have not been altogether inoperative and useless. Such, at least, is the impression made on me by public review notices, private letters, and verbal remarks. Men of high and well cultivated intellects, who have opened my Critique with doubts, and even disbelief of the soundness of its contents, have closed it under a conviction that all the leading notions of Liebig, contested in it, are discredited and overthrown.

My labors, in vindication of physiology, though far from being performed by the arm of Hercules, were honestly intended to aid in clearing that science from the Augean abominations of misplaced chemistry, and humoral philosophism; and I repeat, that I have reason not only to hope, but firmly to believe, that they will more or less conduce to that salutary end. But other causes, not easily resisted, have been in active operation to the same purpose.

To compare great events with small ones, by way of illustration. We are told, I believe truly, by statesmen and historians, that the Roman empire fell by its own weight and its enervating luxuries; and, with equal truth may it be asserted by physiologists and philosophers, that Liebig's Animal Chemistry has been borne down and crushed by the insupportable mass of its own errors and matchless absurdities. Correctly, therefore, may I add, that, had no enemy assailed it, it would have sunk apoplectic, under its plethora of incongruities.

In my defence of physiology against the adulterating influence of Animal Chemistry, I stated sundry reasons for the groundless and almost boundless popularity of that production. And they were all alike factitious and evanescent. With neither nature nor sound reason had they any connection. To two more, of the same description, I shall now refer, premising this special remark, that they have perhaps contributed more to the popularity of the work in the United States, than either or all the causes cited in my Critique. In foreign countries they have little or no influence, Great Britain excepted, where one of them operates to no small extent.

The first of these causes is, that Animal Chemistry was issued from the press, virtually under the patronage and authority of "The British Association for the Advancement of Science." And that this consideration, in the scale of its popularity on this side of the Atlantic, had great weight, will not be questioned by any one acquainted with the condition of the American mind, and the habitual and humiliating propensity we have to overrate and overpraise foreigners, in comparison with our own countrymen, in literature and science, as well as in other manifestations of judgment and taste. Nor is it denied that, in cases like the present, the commendation of the British Association, whether in action or words, deserves to have some weight; but concessions of the kind may be carried so far as to sanction and fortify error, and to amount to an acknowledgment of mastery on one side, and inferiority on the other. And, in literary and scientific concerns, a condition of things, too much of this description, exists between the people of the United States, and those of the enlightened nations of Europe generally, but very especially of the British Isles. Instead of looking *at* those people, as if they were on a level with ourselves, we are inclined to look *up to* them, as if they were above us. Nor is this all that may be correctly alleged, in the case before us.

As respects the subject I am considering, the patronage and favoring decision of the British Association are not of a value altogether peerless, nor even so high as is too generally imagined. It deserves to be here made known and recorded that, on account of the popular nature of its constitution, and the consequent facility of admission into it, with but little discrimination as to the intellect or the information of applicants, that body is composed of elements as dissimilar and non-concordant, as can well be brought together, and much more so than can be always rendered harmonious and satisfactory in their action. On the individual who contemplates its operations through the metamorphosing mist and mirage of distance, it is much better calculated to produce an imposing and authoritative impression, than on him who enters it, passes a few days in the midst of it, and impartially examines it and judges of it *as it is*.

Of the members who compose the British Association, the *few* only are at all extraordinary and distinguished, in either talents or attain-

ments; the *mass* being, like all other masses, of *ordinary* standing, and not a few, perhaps, of those who are ingredients in the incongruous compound even *below the ordinary level*. From a source so exceedingly heterogeneous and opposite in its component parts, no pure and self-consistent issue can be reasonably expected. Certainly such an issue cannot be *always*, nor in a *majority* of cases, attained.

In the transaction of all sorts of business, scientific as well as common, each member speaks, judges, and votes for himself. In their decisions, therefore, the ordinary members exercise far the most influence, because they are greatly superior in number. True, committees composed of respectable members usually report on all important papers, measures, and movements; but the masses confirm or reject such reports or modify them at option.

In a word, though in mathematics, astronomy, geology, and most of the other branches of pure *physical* science, the authority of the "British Association" is deservedly weighty, yet, as respects animal chemistry, the case is widely and essentially different. From naught that I discovered in that body in 1841, when honored with a seat in its meeting at Plymouth, do I feel justified in regarding, with any marked respect, its authority in relation to the merit of Professor Liebig's work. I doubt exceedingly whether *one member in twenty*, or perhaps even *in fifty*, of that division of the Association to which Animal Chemistry belongs, had ever spent a week, a day, or an hour, in a serious and thorough examination of that gentleman's "Report" on the subject. As far as I am imformed, every chemist in the Association, of any respectability, had been a chemico-physiologist long before the Report was read to that body. Hence the partiality and insufficiency of all testimony derived from such a source.

The second reason for the undeserved popularity of Animal Chemistry is one which, as an American, I blush to record. *The work is the production of a foreigner*—a circumstance which, irrespective of merit or any other valuable consideration, strongly recommends it to too many of our countrymen. The reason is plain. *As regards literature and science*, we have not yet, as a people, learnt to know, respect, and appreciate, ourselves. A *colonial*, not to call it a *servile spirit*, accompanied by a virtual acknowledgment of inferiority, so thoroughly pervades and actuates us, that we continue, as we did, in our *provincial condition*, before the swords of our gallant fathers had severed the chains and shackles that enthralled us, to regard Europeans as bearing toward us still, the relation of instructors at least, if not of masters. Deny this, *in words*, as we may, our *actions* testify abundantly to its truth. The result is obvious and humiliatingly discreditable to us. We too often receive their mere *dicta* with the acquiescence and observance which would be due to them only were they delivered to us in the character of oracular responses.

Had "Animal Chemistry," replete as it is with errors and blunders, false reasoning, misrepresentations and gross absurdities, been written by a native citizen of the United States, not only would it have fallen dead from the press, but it would have overwhelmed its author in endless disrepute.

Had the journals of the country, under such circumstances, condescended to notice it at all, it would only have been that they might condemn the whole and treat many parts of it with consuming derision.

Under this head of my subject I shall only add, that as long as we shall tolerate among us this unmanly sentiment of literary and scientific nonage, the Europeans will regard us as a nation of underlings; and they will be justifiable in the measure. He who fails to respect and venerate himself, excites, by a law of our nature, the pity and scorn, or both united, of those who behold him.

But from this degradation we can promptly emerge. A single act of the mind can commence the change, and a few additional ones will complete it. To become actually equal to the *savans* of Europe, we need only *think* ourselves equal to them. The thought will engender the resolution to become so, and the resolution will accomplish the work. No man knows the power of the human will over human destiny—even the destiny of nations, until he has learnt it by firm and persevering trial, and the experience which such trial produces.

In my Critique on the Animal Chemistry of Professor Liebig, I passed no strictures, nor made remarks of any sort, on his "Chemistry applied to Agriculture and Physiology." The reason was, that I had not then acquired an acquaintance with that production sufficiently thorough and accurate to justify me, to myself, in an attempt to form a judgment of it. I acquiesced, moreover, too implicitly in the judgment of others. At present, however, the case is different. The errors and extravagances of Animal Chemistry awakened my suspicions. I have recently, therefore, examined the other work of the Professor with the attention it merits; and I feel authorized to say, that the faultiness of certain portions of it is not only striking, but it is extreme, even in the midst of the faults of its author. The portion of the work, however, by which it is my purpose to substantiate this charge, is, in extent, comparatively inconsiderable. It is that which contains the Professor's descant on "poisons, contagions, and miasms."—Part II., chap. xiii., extending from p. 373 to p. 415.

It is not to be understood that the pages just indicated constitute the only portion of the volume about to be examined, which I consider exceptionable. Far otherwise. Those pages alone contain the matter which I mean to analyse. But the book is exceptionable, deeply exceptionable in every paragraph where it pretends to an exposition of vegetable physiology, whether healthy or morbid. For the principles and laws of the physiology of vegetables and animals are the same. As often, therefore, as the Professor attempts an analysis or explanation of them, he manifests his utter incompetency to the task—his ignorance of the subject being the cause of his failure.

Of Professor Liebig's application of chemistry to agriculture, including his views respecting the food of plants, the improvement, changes, and cultivation of soils, and the fitnesses of given sorts of soil for the production of corresponding sorts of vegetables—respecting these several points and others of a similar description, I shall say but little, because I know but little. I have reason, however, to believe, from information communicated to me, that the portion of his work, here referred to, is highly valuable. But further information from the same sources, induces me to doubt whether the matter be all *original* with the Professor, or whether much of it was not derived from the skilful agriculturists of Germany, France, and Italy; to whom I am told it has been familiarly known, and carried by them into practice, perhaps for ages. In truth, I consider it impossible for Professor Liebig, confined, as he must have

been, since his boyhood, to his closet and his laboratory, to be thoroughly versed in the practice of agriculture. Nor do I make these remarks in any spirit of disparagement toward the work I am examining. My only design is to make it appear, that, instead of being altogether an *electron per se*, as many of his devotees represent him, the celebrated German Professor, like other great modifiers and improvers of science, is deeply indebted, for his knowledge of facts, to the age and country in which he lives. And this is as true of Columbus, Bacon, Newton, and Franklin, as of other more common and every-day men. Had they lived respectively at earlier periods, they would neither have done what they did, nor become what they were; and, had they lived severally at later times, other men of genius and industry would have arisen at the proper dates, availed themselves of existing and favoring circumstances, and become each in his own department, the illustrious discoverers and improvers of the world. So true is it, that, to a great extent, the most renowned personages are the product of the events and general condition of their own times. In no other age but his own, could Washington have become Washington in the new world, or Napoleon, Napoleon in the old.

But to return from this digression. Professor Liebig's chapter on "Poisons, Contagions, and Miasms," must now receive a few minutes of my attention.

In whatever part of it, as heretofore stated, the Professor attempts an exposition of his own views of physiology, whether vegetable or animal, healthy or diseased, he does little else than make bold assertions, without either proof or plausible foundation—indulge in spurious reasoning, or depart from reasoning altogether—dogmatise, as if he were repeating the words of an oracle—and utter notions, instinct with error at times, and again entirely unintelligible. And these are the very elements of *intellectual balderdash*—a repulsive mixture, which men of an ambitious spirit and an ostentatious disposition and manner rarely fail to concoct, when they eagerly descant on subjects of which they are ignorant, as our author does when he treats of any branch of the laws and functions of living matter.

Of the truth of this general statement, my proof shall consist of extracts from the chapter of errors and blunders I am considering. And, if I am not mistaken, they will be found sufficient to justify me in the strongest expressions I have employed. The following are instances where the Professor indulges most freely in open and positive assertions on points, respecting which he neither gives his readers, nor possesses, or indeed can possess himself, a single tittle of proof; and for the elucidation of which he has neither made, nor can make, a single experiment.

"Sausages," says he, "in the state here described [a putrid state], exercise an action on the organism, in consequence of the stomach and other parts, with which they come in contact, not having the power to arrest their decomposition, *and entering the blood in some way or other, while still possessing their whole power*, they impart their peculiar action to the constituents of that fluid." (P. 388.)

Though, to the eye of every enlightened and unprejudiced physiologist, the very aspect of this clause renders it utterly unworthy of analysis, it shall, notwithstanding, receive a concise, but, I trust, a satisfactory one. And, in giving it, I shall, for the sake of brevity, merely *state* my facts

and positions, as fully *acknowledged* and *established* ones, unaccompanied by illustration or proof.

When putrid food is swallowed by man, one of two events uniformly occurs; his stomach rejects it by vomiting, or *removes its putridity*, and converts it into chyme *free from taint*. Chyme, *actually putrid*, or even, I believe, bordering on it, when first taken from the human stomach, has never yet been seen by the human eye. Nor, as every fact bearing on the subject gives us reason to believe, will such a phenomenon be permitted to present itself, until the now-existing laws of our nature shall have been changed. Of this wise and beneficent feature, in the economy of our systems, the final cause is obvious. It is to secure from putrefaction our blood, in common with the other fluids and the solids of our bodies; because that condition of them is not only incompatible *with*, but in direct opposition *to*, the very essence of life.

In open and deadly conflict, however, with this kind and salutary arrangement, made for the benefit of our race, by the ALL-WISE CREATOR, *in the council of Heaven*, is the arrangement fabricated, in his *laboratory and closet*, by the Professor of Giessen, for the benefit of his own dreams and vagaries in chemico-physiology.

By this new system of economy for the human constitution, conceived and developed by the distinguished German, the provision, on the same point, previously established by the Deity (having been discovered by *chemico-physiological* acumen to be wanting in efficiency), is "*nullified*" and "*expunged*" from the Record Book of creation, and a wiser and better provision established in its place!!

In conformity to the principles of the German "new light" fabrication, putrid food received into the human stomach, neither offends the nerves nor disturbs the muscles of that most sensitive, *conservative*, and important organ. For, from the silence of Professor Liebig on the subject, we are not authorized to believe, that his putrid sausages produced, in those who swallowed them, either vomiting or even sickness. They, "some way or other," secret, silent, and unknown, passed *unchanged* and *uninjuring*, through the chyme and chyle-making organs, traversed the lacteals, mesenteric glands, and thoracic duct, entered the blood, their putrid venom still prevailing, and reduced, "by the lump," every putrefiable portion of the body into a repulsive but still *living* mass of corruption!! I say a "living mass," and prove my statement by a further extract.

"All the substances in the body, *capable of putrefaction*, are gradually decomposed, *during the disease*" (the sausage disease), "and, after death, nothing remains except fat, tendon, bones, and a few other substances, which are incapable of putrefying in the condition afforded by the body." (Foid).

Could I find, or even invent, terms sufficiently expressive of *astonishment*, *condemnation*, and withering *derision*, I would offer a remark or two on this extraordinary and revolting paragraph. But, as respects all these points, my resources for expression fail me, and constrain me either to be silent, or to speak inefficiently as to the object in view.

The *entire* body of man in a state of *putridity*, yet, at the same time, in a *living* state, and performing, in that *cadaverous* condition, all the functions subservient to life!! Why, a more insane, preposterous, and repulsive conception never emanated, and never can emanate, from the brain of a bedlamite! Nor do I depict the conception either uncharitably,

or with too much strength of line, or glare of color. I employ not an element of either shape or shading, in respect to which I am not fully justified by our author's representation. In the gentleman's own language:

"*All the substances in the body capable of putrefaction* are gradually decomposed during the course of the disease" (the sausage disease), "and, after death, nothing remains except *fat, tendons, bones,* and *a few other substances,* which are *incapable of putrefying in the conditions* afforded in the body."

To render this clause fairly intelligible, and to give it the least weight or merit, information on two points is essential. I shall therefore ask for it.

What are the "few other parts," beside "fat, tendons, and bones," that do not putrefy during the sausage disease?

And what are the "conditions" necessary to putrefaction, which are not "afforded in the body?"

It is not, I think, possible that, by the "few parts," thus referred to, the Professor means to designate parts so large and numerous, *as the whole digestive apparatus, the circulatory apparatus, the lungs,* the entire muscular system, *and the brain and nerves.* Those parts, therefore, constituting the very seat and stronghold of life and action, are, according to the doctrine under examination, *putrid.* Yet, still does the patient live, breathe, move, feel, and think!! He is, in fact, the most contradictory and revolting of objects—*a living cadaver!!* Deny it who may—be shocked at it who may—avert his face and mind from it who may—such is the disembodied doctrine of the Professor of Giessen. A human being, utterly disqualified for a vital function, still alive, and performing vital functions!! Nor is all yet said that the case admits and requires.

Those parts of the body which I have termed the "seat and stronghold of life," especially the digestive organs and the brain, are among the *most putrefiable* we possess. The nameless "conditions" therefore, so vaguely and uninstructively referred to by the writer, cannot possibly protect them from putrefaction, while other parts, even much less putrefiable, but certainly not more so, undergo that process.

As to my second question, what are the "conditions not afforded by the body?" I regard the very reference to them, by Professor Liebig, as nothing better than a disreputable stratagem, intended and fitted to sustain, among the unthinking, by *pretence* and *mystery,* a notion not sustainable by *fact* and *argument.* All else I shall say respecting it, therefore, is, to offer a defiance to Professor Liebig and his followers, so to expound and define the matter, as to make of it anything more than a pitiful sham,—or a disingenuous *pretext,* where *reality* is wanting. Nor will the challenge be accepted; because the effort to expound would bring discredit on the acceptor. Once more.

Another very unusual feature, in the Professor's sausage complaint, is, that one of the residuary portions of the body, found on inspection, after death, was "fat." Now that "fat" is not so putrefiable as some other parts of organized matter, is true. But it is equally true, that in all sorts of disease, especially of *lingering* disease (and of that description evidently appears to be the disease in question), fatty matter is removed from the adipose membrane with *more rapidity* than, under the circum-

stances of the case, marks the removal of any other sort of animal substance. Hence the leanness produced by all complaints, especially by slow-moving chronic ones. I need not say that this is the result of the great facility with which fatty matter is taken up by absorption. If my recollection, moreover, does not fail me, this ready removal is referred to by the Professor himself, as tributary to the production of animal heat, in cases where little or no carbon is swallowed as food. This feature, therefore, of the sausage malady, demands explanation. Will the Professor have the goodness to furnish it?—and thus remove doubt and mystery.

But wherefore should I consume any more time—rather perhaps, wherefore have I consumed so much already, in discussing a condition of things, which, under the system of laws that governs at present the economy of man, never has occurred, and never can occur? The event would be *miraculous*—something beyond the scope of natural laws and powers, and producible only by *supernatural* ones. As well may the celebrated German assert that he has seen a criminal, after *decapitation* and *disembowelment, walking about* and performing the other functions of life, as a patient thus acting, under the cadaverous condition of the sausage complaint. Blood *coagulating on the headman's block*, is as well calculated to vivify and sustain the system of its owner, as it would be were it a *putrid colluvies* flowing in his blood-vessels. If Professor Liebig believes in either event, he is *no physician*, but an empty *pretender to medicine*.

Such, in truth, I have believed that personage to be, ever since I perused his Animal Chemistry. And a report has lately reached me, tending not a little to strengthen that belief. I am assured by it, on authority which I do not feel at liberty to disregard, that the Professor has never been regularly educated to medicine. He is said to have been bred to chemistry and pharmacy. And, on account of his eminence in *them*, and perhaps in some other branches of science, he received, as a compliment, the honorary title of Doctor of Medicine. That he has never been a *practical physician*, I believe to be certain. From this circumstance alone, his knowledge of the profession must be extremely defective.

I record the report as I received it, without rendering myself positively responsible for its truth. Individually, however, as already stated, I am much more inclined to believe than to discredit it. Nor is the following paragraph less pointed and powerful in the testimony it offers to the same effect.

"The mode of action of a morbid virus exhibits such a strong similarity to the action of yeast upon liquids containing sugar and gluten, that the two processes have been long since compared to one another, although merely for the purposes of illustration.* But when the phenomena attending the action of

* [This is by no means, as Liebig alleges, a new theory, as may be seen most satisfactorily on consulting the English medical classics; for the three great cotemporaries, Sydenham, Morton, and Willis, who lived in London when the plague and other epidemic diseases were rife, all announced, however much they differed on other points, this *zymotic* hypothesis as the basis of their pathology. The early medical writers have very generally directed attention to the analogies which the diseases here referred to have with putrefaction, fermentation, and poisoning. Morton, in his Pyretologia, calls the morbid principle "*fermentum venenatum*," which, as he alleges, communicates its action like a ferment, from which result perceptible transformations (*mutationes*) in the blood, the secretions, and the tissues. EDITOR.]

each respectively are considered more closely, it will in reality be seen, that their *influence depends upon the same cause.*" (P. 389).

As far as I can interpret it, the following is the plain English of this quotation, reduced to practice.

If a "morbid virus," of *any* description I presume (for no discrimination or specification is here made), assail a brewer, it sickens him and kills him, or otherwise, in conformity to the same *chemical* principles and laws which govern his process in the production of beer!! The very conception of this idea by Professor Liebig is certainly one of the coarsest and clumsiest specimens of *soporific thought-brewing* I have ever witnessed. To me it is offensively trashy and nonsensical. Yet is the fortunate author of it destined, in the prophetic dreams and declarations of his advocates and followers, to be the opener of a "new era in the science of physiology!!" A writer who, in one book, makes a man a "*stove*," and, in another, a "*beer-vat*," proclaimed to be the author of a *physiological revelation!!* Shame, shame, on such *bare-faced imposture* and such degrading credulity!!

After a descant on the formation and action of contagion, to my mind as successfully enigmatical and unintelligible as if he had resolutely studied to make it so, Professor Liebig undertakes, in his usual style of dogmatism and positiveness, to show how it is that the vaccine poison and disease are produced, and how that complaint affords protection from the contagion of small pox; two disclosures which, as far as I am informed, no man had ever before seriously essayed to effect. And, had he succeeded in his enterprise, he would have immortalized himself as far above the immortalization of Jenner, as transcendental philosophy is superior to a collection of every-day facts.

In the present case, as in other similar ones, Professor Liebig gives vent to mere assertions, unsupported by either fact or experiment, or even by a single thought characterized by value or even intelligibility. To convince the reader that I neither fabricate nor exaggerate the faults here imputed, I ask his serious attention to the following protracted extract, whose length I find necessary to give a full and fair view of the Professor's effort to *expound*, on wrong principles, or on no principles at all, a subject which, in the present state of medical science, is inexpoundable, and of which he, in common with all other persons, is, of course, abundantly ignorant. If any individual can so far comprehend our author's effort as to interpret it to others, or to derive from it for *himself* either gratification or instruction, I am ready to admit that his penetration, judgment, and susceptibility of pleasure, are of a much higher and more desirable order that mine. To me, the whole concern is a book barred from my vision by a seven-fold seal.

"The action, as well as the generation of the matter of contagion, is, according to this view, a *chemical process*, participated in by all substances in the living body, and by all the constituents of those organs in which the vital principle does not overcome the *chemical action*. The contagion, accordingly, either spreads itself over every part of the body, or is confined particularly to certain organs; that is, the disease attacks all the organs, or only a few of them, according to the feebleness or intensity of their resistance.

"In the abstract chemical sense, reproduction of contagion depends upon the presence of TWO SUBSTANCES, one of which becomes completely decomposed, but communicates its own state of transformation to the second. The second substance, thus thrown into a state of decomposition, is the newly-formed contagion."

Before advancing any further in this quotation, I put the question—Whence does Professor Liebig derive the elements—I mean the *facts*—of this circumstantial and confident statement? Is it from calm and circumspect *observation*, aided by well-devised and accurate *experiments* made on the human body? or from the *toy-shop* of his own inventive imagination, which he is ransacking for matter in support of his hypothesis? And, in default of an answer from the Professor and his adherents, I reply myself, that it is positively and exclusively from the *latter source*. From the former he neither has derived, nor can derive, a shadow of evidence in favor of his notion.

Of the "two substances" of which the gentleman speaks, I fearlessly assert that he possesses no more of real and substantial knowledge than he does of the affairs of the planet Herschel, or of any other more distant and recondite transactions; or if he does, he can impart that knowledge. He can tell us what the "two substances" are, to which he attributes the formation of contagion. To test the knowledge of himself and others on this subject, I unceremoniously ask him, his advocates, and followers, what are those substances? And I will thank them, as my instructors, if they will have the goodness to inform me. But they will not do so, because they cannot.

Finding, however, as I *suppose* (for a *misty supposition* is all I can collect from the expressions employed), that a measure of the sort was necessary, the Professor seems to have made an effort toward the explanation required. But, in doing so, he has given a rigmarole as mystical and disreputable as has ever disgraced the records of medicine. Here it is; and that he may do it entire justice, I entreat the enlightened reader, first to forget or disregard all I have said in relation to it, and then examine it attentively, and judge of it for himself.

"The second substance must have been originally a constituent of the blood; the first *may* be a body *accidentally* present; but it *may also be a matter necessary to life*. If both be constituents indispensable for the support of the vital functions of certain principal organs, death is the consequence of their transformation. But if the one substance, which was a constituent of the blood, do not cause an immediate cessation of the functions of the most important organs, if they continue in their actions, although in an abnormal condition, convalescence ensues. In this case, the products of transformations still existing in the blood, are used for assimilation, and at this period secretions of a peculiar nature are produced.

"When the constituent removed from the blood is a product of an unnatural manner of living, or when its formation takes place only at a certain age, the susceptibility of contagion ceases on its disappearance.

"The effects of vaccine matter indicate, that an accidental constituent of the blood is destroyed by a peculiar process of decomposition, which does not affect the other constituents of the circulating fluid.

"If the manner in which the precipitated *yeast* of Bavarian beer acts (p. 350), be called to mind, the modus operandi of vaccine lymph can scarcely be matter of doubt.

"Both the kind of yeast here referred to, and the ordinary ferment, are formed from gluten, just as the vaccine virus and the matter of small pox are produced from the blood. Ordinary yeast and the virus of human small pox, however, effect a violent tumultuous transformation, the former in vegetable juices, the latter in the blood, in both of which fluids respectively, their constituents are contained, and they are reproduced from these fluids with all their characteristic properties." * * * * * * * *

"The action of the virus of cow pox is analogous to that of the low yeast;

it communicates its own state of decomposition to a matter in the blood, and from a second matter is itself regenerated, but by a totally different mode of decomposition; the product possesses the mild form, and all the properties of the lymph of cow pox.

"The susceptibility of infection by the virus of human small-pox must cease after vaccination, for the substance, to the presence of which this susceptibility is owing, has been removed from the body by a peculiar process of decomposition artificially excited." (Pp. 404-5-6.)

Such is a portion of the *contents* of Part II., Chap. XIII., of "Chemistry applied to Agriculture and Physiology." And sitting, as I am, a *self-constituted* judge of it, I feel it my duty to speak of it, as far as possible, plainly and impartially, and under the guidance of all the penetration, sagacity, and truth, I have at my command. But in doing this I must be brief, for the production before me is utterly unworthy of any waste of time. Nor is such waste at all necessary to do it justice. Three words may be selected which sufficiently characterize it in all its leading qualities—TRASHINESS, UNINTELLIGIBILITY, and NONSENSICALITY! Since the days of Paracelsus, as far as books have informed me, no such contaminating and deteriorating deluge has been poured into the science of physiology. And even the worst of the writings of that wild fanatical enthusiast are far less condemnable. In them, some of the pearls of truth and genius are scattered through the rubbish of unbridled extravagance.

But far different are the passages just quoted from the production of Liebig. They are nothing but a starless mass of error and worthlessness—I should rather say of error and *mischief;* for, that they are calculated to be injurious in their influence, especially on superficial thinkers, is not to be questioned. That hypothesis, which degrades the *body* of man to the level of a fermenting, bubbling *beer-vat*, cannot fail (provided it be believed), to deteriorate his mind, by dragging him, in his own opinion, from the point of elevation in the scale of nature, which the God of nature, by the express terms of his creation, designed him to occupy.

To show how utterly futile and unworthy of credit the assertions of Professor Liebig are, I here ask the question—How does he know that the *non-susceptibility* toward small-pox of *vaccinated persons*, is owing to the "removal of a certain substance from their blood?"

Has he ever analyzed the blood of vaccinated individuals, and found that fluid to be *destitute, in them*, of a substance which it possesses in the *non-vaccinated?* Or has he found the blood in individuals to be, in any respect, different *after*, from what it was *before*, vaccination? Or has he ever made an experiment to that effect? If so, can he tell what that substance is?

Without the least apprehension of being put in the wrong, or even contradicted, I unhesitatingly assert, that our author will not venture to give to either of these questions a *direct* answer in the *affirmative*. From that test of his correctness he will shrink. Yet has he *indirectly* but *substantially* stated each of them *affirmatively*.

He has asserted that, from the blood of vaccinated individuals, some given substance is removed, meaning of course by the influence of vaccination. Yet he does not know that this is a fact. By positively stating it *as a fact*, therefore, he has virtually and voluntarily departed from truth, and wantonly forfeited his credibility as a writer.

Before finally dismissing this subject, I shall subjoin an observation, which I defy Professor Liebig and the entire train of his followers to subvert by a fair adduction of facts.

Notwithstanding the vauntful declaration of his advocates, that the Professor has introduced, or is destined to introduce, a "new era" in physiology, he has not enriched that science *in its true character*, with a single *original* idea, or even with a single *new and amending thought*. He employs only ideas and thoughts derived from others; and these he often so distorts and caricatures as to render them *ridiculous*.

That Professor Liebig may have even greatly and essentially improved the analysis of organized matter *when dead*, I have not denied, or even questioned. But by that, he has added no more to our knowledge of the nature and true philosophy of *living* organized matter, than he has to our knowledge of political economy. If I am mistaken in this assertion, the man who shall detect and demonstrate my mistake, will peculiarly oblige me.

That a work so morally and physiologically corrupting, should, near the middle of the nineteenth century, be written and translated on the east, and republished on the west side of the Atlantic, is not only astonishing, but it is an outrage on the common sense of the medical community. Yet are the preceding extracts taken from the "FOURTH AMERICAN edition, with large additions!!" It will be borne in mind that all my remarks on Animal Chemistry relate exclusively to its physiology, healthy and morbid. Its *genuine* chemistry, I repeat, which I doubt not is of great value, I cheerfully leave to the appreciation of the votaries and competent judges of that important branch of science. One more blunder of our author in medicine, and I shall take leave, for the present, of him and his writings.

"Chemical actions are propagated in no organs so easily as in the lungs; and it is well known that diseases of the lungs are *above all others frequent and dangerous*." (P. 406.)

This passage is unworthy of comment. Nor could any comment of mine add to the flagrancy of the error and absurdity which it already exhibits. It is not true, as is familiarly known to every enlightened physician, that "diseases of the lungs are *above all others frequent and dangerous*." On the contrary, it is, most discreditably, the reverse of truth. Taking the annual average of mortality in every known climate and country, hot, cold, and temperate, savage and civilized, it will be found that death from affections of other organs, compared with those from diseases of the lungs, stand in the proportion of perhaps *ten* to *one*. Assuredly it stands in a very *high proportion*. By the warmest admirers and most devoted partisans of the German Professor (provided they be experienced and well-read practitioners of medicine), this truth will not be contested. Include in the estimate the common affections of the lungs called *colds*, which are accompanied neither by mortality nor danger, and even then the number of pulmonary complaints will fall very far short of the number of those whose seats are in the other organs of the body. In all matters, therefore, touching the functions, phenomena, and laws of living organized matter, whether in health or disease, Professor Liebig is the very *opposite of sound authority*.

Having closed my remarks on the hypothesis of the celebrated German

savant, it is my purpose now to examine, to some extent, the doctrines and opinions of the Chemico-physiological School of Great Britain.

Of that school the leading members, to whose writings I shall chiefly refer, as containing the creed and orthodoxy of the sect, are Drs. Roget, Carpenter, and Prichard—gentlemen and scholars who, as *medical philosophers*, are immeasurably pre-eminent over Professor Liebig. I shall, accordingly, feel myself bound to treat their productions with a corresponding degree of observance and respect—a measure that will afford me much gratification.

To attach to my disquisition all the collateral weight and authenticity in my power, I shall, perhaps, occasionally back and fortify my own sentiments, on the general subject, with those of Tiedemann and Müller, two of the ablest and most accomplished physiologists of the day. And, though I regret to know that, hitherto, the last-named gentleman has deviated from himself by assuming and occupying a sort of *quarter-way* ground between the chemico-physiological school and that of *true* physiology, I have reason to persuade myself that that deviation will not much longer endure. Influenced by the masterly and independent character of that Professor's mind, and the ardor, honesty, and efficacy of his inquiries, I do not permit myself to doubt that he will yet disentangle himself from early prejudice and partiality, and become the champion of a doctrine in harmony with nature and worthy of his own exalted reputation.

Experience and observation have long since taught me that failure to be successful and useful in discussion is much more frequently owing to a want of clearness and accuracy in definitions and statements, and perhaps also in general phraseology, than to a want either of knowledge or native ability.

Under the influence of a want of such perspicuity and exactness, it is scarcely possible for the parties to the discussion to ascertain, with precision, the actual points which they mutually design to establish or overthrow. And it is unfortunately true, that both in single terms and entire expressions, intended to indicate the *specific* cause or agent of vitality, more ambiguity perhaps is involved than in those indicative of any other point in natural science.

To the cause of vitality I have affixed the term "specific," for a reason which is deemed unexceptionable and important. In its effects, that cause (if such *can* be the case) is more entirely distinct and apart from everything else, and, in its operations, more essentially identified with *itself alone*, than any other agent that can be named. And this is as true of it, in its relation to every form of *chemical agency*, as it is with regard to that which controls the movements of the heavenly bodies, or fits the sun to be the fountain of light. It is equally itself alone and distinct from them all. This, however, instead of being merely stated by me in the way of assertion, is the position which I stand pledged to make good in opposition to the tenets of the British chemico-physiological school. Nor does this constitute the whole amount of my engagements.

I further pledge myself to avoid, as far as possible, the ambiguity referred to, as well in my own terms and forms of expression, as in my interpretation of the terms and expressions of others. And my object in all this will be, not what too many controvertists aim at—to envelope my subject in shadow and mystery; but to elucidate it, and thus arrive, if practicable, at some definite and conclusive decision.

I have promised an examination (and it shall be a strict as well as a fair one) of the "doctrines and opinions of certain" distinguished chemico-physiologists of Great Britain, who are not only the leaders but the acknowledged teachers of the school they belong to—virtually if not formally dictating to their fellow-members and followers, the creed they profess and the orthodoxy they maintain. A few words more in the form of preliminary explanation.

It is my intention to make my contest with those writers both open and honorable; that, as far as I am concerned, truth alone, and neither finesse nor expedient, may arbitrate between us. That they may, therefore, have as fair a field for proceeding against me as I have for my movements against them, I deem it right to communicate to them as full and explicit a knowledge of my physiological profession of faith as I possess of theirs.

I wish them, therefore, to understand that, to the ultimate extent of the meaning of the terms, I am a believer in a *vital principle*, which, as already intimated, I regard as a specific kind of *imponderable matter*, as indispensable to vitality as carbon is to carbonic acid, azote to ammonia, or hydrogen to water. This *materia vitæ* is an essential element of living organized matter of every description, whether vegetable or animal. Nor do I hesitate to express my persuasion of at least the strong probability, that, according to the time-worn scholastic dogma, ponderable matter, in and of itself, is altogether *inactive*—that it is spell-bound by a *vis inertiæ;* and that the sole cause of its action is derived from the *imponderables*. Without them, material creation would be motionless.

I further believe in the existence of what may be termed *vitallizable* matter, because it is participant of life, and indispensable to it. Of this, there are four kinds—carbon, oxygen, azote, and hydrogen. Of these, there is good reason to believe that two or more kinds are essential to the constitution of every sort of living organized matter; because they are found in every sort. To form vital matter, let the requisite number of these vitallizable elements be brought together, suitably arranged, and duly impregnated, or, in the appropriate way, united with the vital principle; and the work is done. Some form of living organized matter is the product; but, in and of itself, each kind of vitallizable matter is *lifeless*.

Nor am I convinced that *formal parental* influence is indispensable to the formation of this union. Against the doctrines of *spontaneous* and *equivocal* generation, I well know the prejudices and antipathies that exist. I have witnessed many a copious and boiling stream of verbiage let off in opposition to them, at times with mighty vehemence and clamor. But the noise was as empty as it was angry and obstreperous. To my mind, it never conveyed against the doctrines even the shadow of a solid argument.

I do not positively aver that the doctrine of spontaneous generation is true; but I do aver that, in the belief and defence of it, there is nothing unphilosophical and irreligious, immoral or unbecoming. Yet have many individuals, in whose minds more liberal views should have existed, been profuse in charges to this effect against its adherents and advocates. Nor do I hesitate to add, that facts exceedingly striking and apparently substantial, and arguments highly plausible and difficult of refutation, if not altogether irrefutable, in favor of the doctrine, might be easily adduced.

I also believe that the vital principle enters the system through the lungs, and there becomes an element of the arterial blood. Having thus communicated life to that fluid, it accompanies it in its circulatory course, and vitallizes, in its round, every portion of the body. Hence is respiration the only *truly* vital function of the system; because it is the source of vitality to others, as well as to itself.

I believe again that, by means of the great diversity of the organization and structure of the various tissues and organs of the body, the *materia vitæ*, as a *single* agent, is capable of producing the numerous sorts of action and their effects, that exist in the body. Be its cause or spring of action what it may, that every organ conforms, in its function, to its organization and structure, is an axiom in physiology not to be questioned.

In illustration and proof of this, testimony in abundance will be hereafter adduced. At present I shall only further say, that, according to my information on the subject, the notion that there exists a "*vita propria*," as it is termed, or a specific form of *vital endowment*, for every different organ of the body, suited to, and productive of, its peculiar mode of action, though once entertained, is now rejected by every able and enlightened physiologist. Confident I am that it ought to be rejected, as will more fully appear in the sequel of this paper. It is not a "vita propria," but an *organizatio propria*, that fits an organ for specific action. The vital endowment of the lungs, the liver, the kidneys, and the brain, is the same. But their forms of organization are different, and hence their adaptation to the performance of different functions.

I believe that mere arrangement, combination, or juxtaposition, cannot infuse new and vital properties into single and fundamental molecules of dead matter. I cannot, therefore, concur in the notion entertained by many, that organization is the cause of life. Of this, the very converse I am confident is true. Life is the cause of organization. On this topic, also, a few further remarks will be made, on a subsequent occasion.

I believe that in living matter, when in a healthy condition, the vital principle prevents entirely all sorts of chemical action, or overrules and directs it to its own purposes. In the economy, therefore, of living organized matter, chemical affinity, as a separate and independent power, has no more influence or agency, than the laws of vitality have in a block of granite. I mean, of course, in the *active* economy of living matter—in all the operations of the vital *plastic* power—in giving form, size, and color—in nutrition, secretion, the diversities and aptitudes, in structure and endowment, of the several tissues and organs, and such other processes as the chemistry of the laboratory and of exterior nature in general never performs.

I believe in the existence of a peculiar and distinct kind of attraction, entitled to the name of *vital* attraction. And this is as different from chemical attraction, and the attraction of cohesion, as they are from each other. Of this, also, an example will be given.

Finally; I believe the notion which assigns to chemistry and vitality a joint agency in the performance of any of the functions of living organized matter, to be, if possible, even more incongruous, than that which assigns them to chemistry alone. That it is directly at war with common sense is indubitable. The reason is plain. By every person, who is in the slightest degree acquainted with the subject, it is acknowledged that the laws of chemistry are essentially different from the laws of vitality, and, in most respects, directly opposed to them. And both sources

of influence act on and with matter, *not in masses*, but in *atoms*. If both be concerned, therefore, in the function of the same organ, at the same time, they are both acting at once on the same molecules of that organ. They must be acting, moreover, each according to its own laws, which, as already observed, are widely different from, and, in many respects, the very opposites of, one another. From a condition of things thus irreconcilably conflicting, a chaotic issue would be necessarily produced.

Such are some of the leading tenets of my creed, in relation to the cause of vitality and the constitution of living matter. And they are irreversibly at war with those of the chemico-physiological school. Hence, that either the members of that school, or myself, are deeply immersed in error and its consequences, will not be denied. With a view, therefore, as far as my resources may avail me, to decide the question, on whose side (that of the chemico-physiological school, or that of the purely physiological one, to which I myself belong), do error and its influences lie, a further inquiry into the subject, conducted in a different mode, shall now be instituted. But, previously to the commencement of this fresh form of disquisition, a few preliminary remarks will be in place.

(*To be continued.*)

ART. VI.—*Case of Lithotripsy*. By ALBAN GOLDSMITH, M. D., late Professor of Surgery in the College of Physicians and Surgeons of New York, and in the Medical College of Ohio.

MR. A., æt. 81, of strong, robust frame, applied, March 1st, 1843. His occupation, during the early part of his life, had been that of a butcher. He stated that, about forty years before his application to me, his lower extremities had become paralyzed, the bladder also being affected; but that they had gradually recovered their functions in the two succeeding years; and that now there remained only occasional "creeping" sensations in his hips and thighs. He had suffered from pain in micturition for forty years; and from the commencement of this symptom to the present time, he had at different periods discharged in his urine small white calculi. He had also on several occasions suffered from obstinate retention of urine.

He complained, at the date of his application, of inability to retain his urine for more than half an hour, and of great pain in micturition. His urine proved, on examination, to be alkaline, and to be loaded with pus and mucus.

In attempting to introduce a catheter (No. 8), I found a stricture two and a half inches from the glans penis; and, although I succeeded in passing this point with an instrument (No. 6), yet it became entangled at the neck of the bladder. This difficulty was overcome by withdrawing the catheter and directing its point to one side, when it passed readily into the bladder, coming in contact with one large stone and several smaller ones. The only way in which I can account for the entanglement of the instrument at the neck of the bladder, is on the supposition

that the third prostate was enlarged; and that, as the patient had for some years been in the daily habit of introducing a catheter to draw off his water, he had perforated this portion of the gland. That the end of my instrument had passed into the laceration thus made, I inferred from its seeming to be bound down so as to prevent a free exploration of the bladder. Upon examination through the rectum, however, I could not discover that the prostate was enlarged, though the bladder felt much harder than is natural. Hence I inferred that its coats were considerably thickened—a fact which might account for the indistinctness of the prostate.

Treatment.—I treated the stricture by dilatation; and on the twelfth of March, I began the operation of lithotripsy. I introduced a catheter, and through it filled the bladder with warm water. When the catheter was withdrawn, the injection passed out rapidly, bringing with it shreds of ropy purulent substance, resembling very much the broken down integument of ulceration. These masses were saturated with sandy sediment. Mr. A. told me that he had often noticed them, as they would sometimes get into the urethra; and that he could then get them out, by pushing the stone out of the neck of the bladder, so that the urine could pass more freely.

I next introduced a lithontriter, No. 10, (Heurteleup's); and having seized a stone, broke it. This was done easily and without pain. A large quantity of detritus, mixed with the fibrinous masses just mentioned, passed during the same day. I finished the operation in twelve sittings, each of which was at my office. Some of them were on successive, and some on alternate days. The patient complained of no pain, except in two instances; and then it was caused by the lodgment of fragments at the strictured portion of the urethra. These were removed by the long urethra forceps. There was no increased irritation of the bladder, during any part of the treatment. The pain in urinating ceased with the passage of the last fragment, on the second of April; and the patient congratulated himself upon the great relief which the operation had afforded.

My attention was then directed to the relief of his remaining symptoms. He was very old; and his bladder had so long been subjected to the action of the stone, that it was evidently permanently diseased. He could not retain his water long, say from three to six hours at most.

I saw him occasionally after this for the purpose of keeping the stricture dilated, so that the fibrinous masses, which were still formed, though in less quantity, might find an exit. He was, by this means, kept comparatively comfortable, with the ability to retain his urine gradually increasing; but, some time in June, in descending a flight of steps, he fell, and bruised his back and hips. This injury confined him to his room for some time, and he left it a good deal debilitated. He rode out occasionally, and seemed to regain his strength; but he was soon attacked by the epidemic influenza, so prevalent at that time in our city. He was taken with chills, followed by fever and profuse perspiration; and he continued in this condition for eight or ten days, when, under the judicious treatment of his family physician, Dr. Cockroft, his chills subsided. But they soon after returned; and our patient died apparently of old age and exhaustion, about the first of August.

Post-mortem examination.—The abdomen alone was laid open, and the examination was restricted to the urinary organs. The kidneys were slightly atrophied, with a pelvis somewhat enlarged. The ureters were

dilated to twice their natural size. The left ureter and pelvis were filled with very thin pus. The bladder was removed along with the ureters and a portion of the penis. The coats of the bladder were found nearly half an inch in thickness; and the inner surface was covered with adherent fibrous masses, presenting the appearance of a cancerous uterus. This was the substance which, detached and lodged in the urethra, so often produced retention of urine.

The prostate seemed to be nearly obliterated; but over it and at the neck of the bladder, I found some of these adherent masses, projecting from the surface about half an inch. To this cause is to be ascribed the entanglement of the instrument in the operation.

The urine was highly acrid and ammoniacal, mixed with pus and floculi. No fragments were found.

The urethra was irregular, and it presented some induration at the strictured part.

Remarks.—This case illustrates several important points in the pathology of the bladder.

In the first place, there was no paralysis, which impaired the functions of the bladder. Now, whenever the normal powers of this organ are impaired, it approaches in its character, in a greater or less degree, to a non-vital cavity; and hence it allows, as soon as the urine collects in it, chemical action to take place. Calculous matter, the result of such action, is deposited, which, coming in contact with the coats of the bladder, is a cause of irritation; and thus is the original disturbance increased. This patient had suffered from severe attacks of retention of urine, which also impaired the power of the bladder, and aggravated the disease.

Here, then, we have sufficient cause to explain the thickening of the coats of the bladder by the deposition of lymph between the laminæ. Thus is the vitality of the vesical organ impaired; its muscular power is lessened by the hypertrophy, becoming, in fact, an inert receptacle for the urine, instead of a sensitive organ; and so far from presenting, in virtue of its vital power, chemical action, it is ready, from its muscular energy, at any time, to contract and expel its contents. These morbid actions continue; the parietes become more and more thickened; coagulable lymph is effused from its inner surface; and as this last is infiltrated with calculous matter, we thus have a nucleus for a stone.

I find that this thickening is a very common occurrence in strong, robust constitutions; and that when the bladder gets into this condition, it loses much of its sensibility; and, indeed, is so little irritable, that injections and instruments can be introduced without producing pain or inflammation. On the other hand, when the constitution is not so robust, or is associated with a scrofulous taint, and instead of thickening the bladder secretes large quantities of pus, it becomes very irritable, and it is with great difficulty that you can introduce an instrument or an injection, without producing an increase of irritation.

In this case the bladder was not irritable, nor did the operations produce any irritation. The only pain or difficulty was in the passage of the fragments through the strictured part of the urethra. Their passage in two instances was facilitated by the long forceps.

There was great pain in this case from the stone getting into the neck of the bladder, and the patient was sensible of the diminution of pain on the passage of successive portions of detritus.

It might be said that in this case the operation hastened death; but this could not be, since there was no evidence of an increase of the disease of the bladder; while, on the other hand, after the operation, the urine was discharged at longer intervals, unattended by pain. There was no pain or fullness over the region of the pubels; nor was there any sign of inflammation in the region of the bladder. He continued to improve gradually and steadily for more than two months, both in his general health and in that of his bladder. It is hardly possible that a mechanical injury, especially in such an organ as this, should remain dormant, and only develope itself after the space of two months or more. His extreme age, the constitutional debility which must inevitably have been brought on by the long continuance of the disease, the depressing influence of the injury he received; and, above all, the epidemic influenza, which, unaided, carried off almost every very aged person that it attacked, are causes sufficient of themselves to account satisfactorily for his death.

It would seem that if, as in this case, a stone can be broken in a bladder as dreadfully diseased as it was, without producing inflammation, then there are few cases in which simple disease of the bladder need interfere with the application of lithotripsy. It is absolutely necessary for the successful performance of this operation, that the surgeon should study the habits of the bladder; and thus learn by a frequent introduction of instruments, how much may be done with impunity. The great drawback to its success in practice is the want of care and gentleness in manipulation. In hospital practice, whence the most of our statistics are drawn, operations are frequently performed before the bladder has yet recovered from the irritation of a preceding essay, prompted, no doubt, by the presence of those who come to witness the surgeon's skill. Such practice is not only dangerous to the patient, but as it brings into disrepute an operation which need in no case hazard life, it is actually suicidal.

ART. VII.—*On the Influence of Opium upon the Catamenial Functions.* By JAMES MCCUNE SMITH, M.D., of New York.

PEREIRA observes, "that we have little positive information as to the effects of opium on the reproductive organs of women. It is said that the catamenia, lochia, and secretions of milk, are unaffected by it;" whence he infers "that its use in the female is not likely to be attended with retention of the uterine or mammary functions."*

The following cases, which have fallen under my notice, within the last four years, give uniform evidence contrary to the above view.

CASE I. E. L——d, a middle sized, plethoric young woman, *aet.* 24. In 1837, having procured an abortion by means of some powerful drug, she sent for a physician, who gave opiates in the course of his subsequent treatment. From that time until April, 1843, she resorted daily to laudanum, and gradually increased the dose to four ounces per diem.

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* Mater. Med. and Therap., vol. ii., pp. 700, 781. Philad. Edit., 1843.

Her appearance last April, was as follows: look, stupid; complexion, florid; person, fat; bowels, regular; and she ate very little food. Her catamenia, which had been regular until January, 1837, have never appeared since. I believe she is now in the Magdalen Asylum at Yorkville.

Case II. E. M——l, *aet.* 23, of middle size and sanguine temperament. About three and a half years ago, during an attack of acute rheumatism, laudanum was freely administered to her. During her recovery, and until March, 1843, she continued the daily use of the tincture of opium; and from 1841 to 1843, she drank from four to six ounces per day. In March last, she had clear complexion, ruddy cheeks, good general health, bowels regular, and ate little food; but from the date of her rheumatism, until March, 1843, her catamenia did not appear, notwithstanding she had been perfectly "regular" until that attack. She went into the country in April, 1843, where she could not obtain any laudanum; but she applied to a physician, who prescribed an emmenagogue, which was followed, at first, by a slight, and subsequently, by several profuse, but regular, returns of the catamenia. She has recently (July 20th) returned to this city, and has resumed a very moderate allowance of laudanum.

Case III. S. S——h, a short, fleshy, plethoric young woman (colored), *aet.* 26. Until July, 1837, she had menstruated regularly; but, at that date, she commenced the daily use of laudanum, to allay the pain of *onychia maligna* of the great toe of the left foot. She continued drinking from two to four ounces of tr. opii, per diem, until June, 1842. During these five years, although her general health remained unimpaired, her catamenia did not appear. At the last-named date, she was cured of the *onychia*, by an operation. The succeeding month, and every month since, the menses have punctually appeared, without the aid of any medicine.

Case IV. R——n, *aet.* 25, of middle height, robust, sanguine temperament: has a child four years old. About two years ago, while confined to her room by a fracture of the left tibia, she commenced using opium to relieve pain and restlessness. She has sometimes used twenty or thirty grains of opium per day, for two or three months steadily; at other times, she quitted the stimulant entirely, for the same length of time. While she continued using the opium daily, her catamenia did not appear; but, on forsaking the drug for a month or two, she becomes "regular" without an emmenagogue. Her general health is good, and she has not been constipated at any time.

Case V. E. R——, *aet.* 18, a young, unmarried female (colored), applied to me, in May or June, 1841, for relief from an increase in the quantity and frequency of the catamenia. She was pale, thin, and of the nervo-sanguine temperament. Pulse frequent, but soft; head dizzy. She has a sister and brother; both are married and have progeny. There were no coagula in her uterine discharges, and except physical weakness, her general health was good. Having tried the ordinary treatment without success, I gave her a combination of acetate of lead, camphor, and *opium*, in minute, frequently repeated, doses. I commenced the use of this combination on the fourth day of the persistence of the discharge, and continued the remedy until the discharge ceased. She was greatly relieved by this practice. Shortly afterwards, on leaving the city, she procured a copy of the prescription; and, without my knowledge, continued using the pills (as her sister informed me lately) nearly

a year. The catamenia gradually diminished, and, at the end of a year, entirely ceased. She was subsequently (Sept. 1842) married. Her general health remains good, her person is thin; but menstruation has not recurred, nor has she yet become pregnant. Since abstaining from the pills (Aug. 1842), she has repeatedly resorted to emmenagogues, prescribed by various physicians, but without success.

Remarks.—The first four of the above females are courtezans. But their vocation cannot have been the cause of the arrest of their menses; for, I can say, from a somewhat extensive experience in the Lock Hospital at Glasgow, that this unfortunate class have no peculiar liability to suppressio mensium. E. M——l (case II) informed me that she is acquainted with several opium-eaters, in all of whom the menses have been arrested, as they believe, by opium.

Without assuming from so few cases, that the daily use of opium will *always* arrest the catamenia, I submit them, in the hope that they may attract the attention of medical men to whatever facts may be within their reach. It will be interesting to ascertain, in relation to married women, who have borne children previously to a resort to opium-eating, whether this habit arrests their catamenia, and their capacity for conception; or, whether it arrests the one and not the other. It may also be worth the inquiry, whether opium, in skilfully regulated doses, may not be used as a means to bear women safely through the critical disturbances which occur at the "change of life."

One circumstance is common to all the above narrated cases, viz., the arrest of the catamenia was not followed by the vital disturbances which attend suppression from other causes; or, in general terms, opium, in daily doses, arrested uterine periodicity without disturbing other ordinary vital functions. Here we have an analogy with other well known effects of opium; for this drug, to a great degree, arrests hunger, which, in the present state of civilized society, is a *periodic habit*, for it is seldom allowed to become an actual *want*. In a therapeutical point of view, opium exerts an almost uniform influence as an arrester of periodicity.* As a narcotic, its influence is not uniform, for it does not always produce sleep; and I am inclined to the belief, that when sleep does follow its exhibition, it is rather a consequence of the absence of (an arrested) paroxysm, than of a direct narcotic influence.

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* Trallian (vide Senac on Fever, p. 220. Caldwell's Trans.), Sydenham (Rush's Edit., p. 90, et passim), Fordyce (Dissert., p. 278, Boston Edit., 1815), Armstrong (on Typhus, p. 365, London, 1817), M'Causland, Breda, and Thomann (Copland's Dict., vol. i., p. 994), and Parry (Amer. Journ. Med. Sc., July, 1843, p. 31), have administered opium to arrest the recurrence of *periodic* paroxysms of rigors, febrile exacerbation, delirium, and congestion, and with general success.

MONOGRAPH.

ART. VIII.—*An Essay on the Origin of Syphilis.* By JOHN LE CONTE, M.D. Read before the Georgia Medical Society, at Savannah, May 18th, 1843.

ALTHOUGH it may be considered *practically* unimportant to investigate the natural history of those diseases which are usually denominated as specifically contagious; yet it affords a topic of curious research to the theoretical inquirer and the naturalist. It is impossible to imagine that the first created individual was born with all this catalogue of loathsome diseases upon him, and that they were thus entailed upon all future generations; and yet they seem to be produced by no cause except a specific contagion. Almost every person is obnoxious to them upon the application of this cause; and this liability appears to be little affected by constitution, age, habits of life, climate, or other external circumstances. We know of no distinct and appreciable cause now in operation, which could, in the first instance, have generated them. These remarks apply to the small-pox and the measles, which were first known to the Europeans about the middle of the sixth century, when the former raged with violence in the besieging army at Mecca; and they apply, perhaps, still more remarkably to the case of *syphilis*.

Alexander Benedictus, a Veronese physician, was the first to admit, as a contagious principle, *a venereal taint*. This was embraced by Fernel, and received the name of *lues venerea*, poison, venereal virus, etc. (M. Ricord's Treatise on Ven. Disease. New Trans. New York: 1842. p. 7.)

There is no subject in the whole range of medical science, which has given rise to more visionary and unprofitable discussion, or which has been more mystified by disputation, than the origin of syphilis. This perplexity extends even to the derivation of the name. Thus Dr. F. Swediaur derives the term from the Greek, σῦς, a hog, and φιλια, love (impure or foul connection). (Vide Swediaur's Treat. on Syphilis. Hewson's Edit. Philad., 1815. p. 246.) Others derive it from the Greek σιφλος, formed by syncope from σιπαλος, "deformed," "ugly," "ridiculous from personal defect," etc. (Vide Hooper's Med. Dict. 4th Am. Edit. New York: 1839. Art. Syphilis. Also Dunglison's Med. Dict. 3d Edit. 1842. Art. Syphilis.) By adopting the latter etymology, our distinguished lexicographer, Dr. Noah Webster, has been induced to discard the ordinary method of spelling the term; for he has been compelled, by the principles of philology, to make SIPHILIS the correct orthography! This error has crept into all the numerous editions of his excellent Dictionary, in the abridged octavo form, up to the year 1842. However, in the *larger* edition, in two volumes, published in 1841, the corrected orthography is given, and the origin of the term is accurately referred to the poetic invention of Fracastorius. The opinion of the learned Dr. Good on this point is unquestionably, in the main, the correct one. He remarks that the specific term syphilis "was probably invented by Fracastorius, about the close of the fifteenth century, from the Greek συ and φιλέω, importing "mutual love;" for such is the title by which he has designated his celebrated and very elegant poem upon this very

inelegant subject." (Vide Study of Med. Doane's Edit. New York: 1836. Vol. II., p. 91). Whatever objections may be raised against this opinion in a philological point of view, one thing is clearly manifest, viz., that the term was actually fabricated by Fracastorius at the above-mentioned period; for our poet describes his fictitious hero, SYPHILIS, as having brought down the disease upon himself and the world at large, as a condign punishment for having insulted Apollo, while tending the flocks of King Alcithous. (Vide Dr. Good, op. cit., vol. ii., p. 91.) Dr. A. J. L. Jourdan observes, on this point, and, perhaps, very justly, that "from this, appears the futility of searching for the etymology of a word forged in the imagination of a poet." This derivation was suggested by Sauvages. It is not, however, I think, beyond the province of reasonable conjecture, to suppose that Fracastorius was guided by some classical analogy in the selection of a term. (Vide Historical and Critical Observations on Syphilis, by A. J. L. Jourdan, M.D. Translated from the French by R. La Roche, M.D., as appended to P. J. G. Cabanis' Essay on the Certainty of Medicine, translated from the French by the same. 8vo. Philad., 1823. P. 41.)

This specific term is, therefore, borrowed from the name of the unfortunate hero of Fracastorius, who was first afflicted with this curse in the shape of disease. He is represented thus:

> ———" ostendit turpes per corpus achores,
> Insomnes primus noctes, convulsaque membra
> Sensit, et à primo traxit cognomina morbus:
> SYPHILIDEMQUE ab eo labem dixêre coloni."—(Ibid.)

It is not a little singular, and it is certainly very remarkable, that the ravages of such a horrible malady should have the effect of inspiring the imaginative with *rhapsodies poetic!* And yet we all know that Fracastorius, who was an eye-witness of its first and most hideous ravages, has left us a most splendid poem in Latin hexameters, exclusively devoted to this *unique* theme!

Soon after syphilis appeared, the national genius of the French exercised its natural gaiety in the composition of ballads and allegories, such as the ballad of John Droyn, of Amiens, published in 1512; and the allegory of John Lee Maire, which appeared in 1520. (Jourdan, op. cit., p. 46.) Indeed, even so late as 1841, M. Barthelemy published an elaborate poem on *syphilis*, in French, at Paris! He truly says, that all the subjects have been *deflowered* (used up), and in seeking for a *virgin soil*, he has selected *syphilis!* (Vide New York Med. Gazette, 1842, vol. i., p. 393.) Assuredly, no *other disease* with which humanity is afflicted, has ever inspired the enthusiastic with such bewildering romance, and invested the subject with the charms of such delicious harmony!

I shall now proceed to investigate the more difficult problem, viz., *the origin of syphilis*. The exclusively *practical* physician may attach little value to the origin of diseases, and may disdain to trace the geographical march they have observed on the surface of the globe. But he who does not consider medicine merely as a profession—the most noble and honorable of all—but as an assemblage of scientific knowledge, endeavors to acquire precise notions of the history of those maladies which are presumed to have been little known to the ancients, or are more widely

disseminated than formerly. Notwithstanding the agitation of the question of the origin of syphilis for more than three centuries, we are far from having arrived at a satisfactory solution of a problem no less interesting to medicine than to anthropology.

There are three principal questions in dispute: namely; *First*, was *Syphilis* known to the ancients? *Secondly*, was it imported from America to Europe? And, *Thirdly*, If not imported to the old continent, when and where did it originate? These questions we shall discuss *seriatim*.

1. *Was Syphilis known to the ancients?* As a preliminary matter essential to a correct appreciation of this question, it is necessary to maintain a clear conception of what is meant by syphilis. Now, it appears to me that this has been neglected by almost all writers; and the fact may serve to explain the origin of the contradictory opinions which have been successively advanced on the subject. It is well known that it is only very recently, that the admirable experimental researches of M. Ricord, have demonstrated the *non-identity* of gonorrhœa and syphilis. R. Carmichael says, that "*gonorrhœa* was not described as a symptom of *syphilis*, till half a century after the introduction of the latter into Europe." (Essay on Ven. Dis., Phil., 1825, p. 81.) I shall, therefore, use the term in the restricted applications which modern investigations have imposed upon it; and which are familiar to every member of this society. Some of the partisans of the antiquity of this disease, maintain that, if it could have originated from the licentiousness of the fifteenth century, it *might have been* generated a thousand times *de novo*, during the history of the world, by impure sexual intercourse. This conclusion is most unquestionably true. It is not denied, that there has always existed a *primeval capacity in the human system, under certain circumstances, of generating this malady;* but this does not touch the question at issue. For the inquiry arises—*Did this efficient concurrence of circumstances take place among the ancients so as to give origin to syphilis?* This is a point of strictly *historical research*, and which is, therefore, intangible by any system of *à priori* reasoning. We shall pass over the opinion of Raymond and the learned authors of the Asiatic Researches, who maintain that the syphilitic virus has always existed, though in a less degree, among the Chinese and Bramins in the warm latitudes of Asia. So also, with regard to the assertion of John Reinhold Foster, and Captain King, that it has always existed among the Otaheiteans and other inhabitants of the South Sea Islands. (Vide Dr. A. J. L. Jourdan's Hist. and Critical Obs. on Syphilis. Translated by Dr. R. La Roche, as appended to Cabanis' Essay. Philadelphia, 1823, p. 12, et seq. Also, pp. 105 et 107.) The historical, chronological, and traditionary evidences on which they rest, are too meager and incomplete, to warrant any conclusive or satisfactory deductions. We shall, however, confine ourselves to the question of the *European* origin of syphilis. I trust that it will be made manifest in the sequel, that the supporters of the antiquity of this disease have grounded their arguments principally upon the latitude afforded them by the *indefiniteness* of the term *venereal;* and more especially, upon the presumed *identity* of gonorrhœa and syphilis; which are *now* known to be two essentially distinct affections. In the strict etymological interpretation of the term *venereal*, there can be no question that *such diseases* must have been known and recognized from the remotest antiquity.

M. Ricord judiciously observes, in commenting on the memoir of M. Donné, "that there is a want of precision which may lead to error; for

in syphilography, the term *syphilitic* cannot be confounded with that of *venereal*; and yet they are used by Donné in the same sense." (Vide M. Ricord's Treatise on Ven. Disease. New trans., New York, 1842, p. 34.)

It would, indeed, be an unreasonable and unwarrantable exception to a universal law of nature, if the *genital* system of man were not liable to a variety of diseases. Excesses in the pleasures of Venus, have produced at all times, as they do at present, certain forms of gonorrhœa or blennorrhagia, and their baneful consequences, as excrescences of various species on the genitals, and other accidents arising from them.

There can be no question but that prostitution has, in all ages, been productive of diseases of the generative organs; and that they always have had a bad reputation, is manifest from the satires of Horace, Juvenal, and Martial, among the Romans, and the works of the libertine Francis de Villon, and the Latin ode, addressed to Priapus, by Pacificus Maximus, among the authors of the middle ages. From the reign of Charlemagne, *houses of pleasure* were established in most of the large cities of Europe. Jane, countess of Provence and Queen of the two Sicilies, who was celebrated for her amorous adventures, established in the city of Avignon, in 1347, a house of prostitutes, with an ordinance, compelling them to submit to examination every Saturday, to ascertain whether they had contracted any lewd disease. Similar regulations existed in London as early as 1162, according to Mr. Beckett. (Jourdan, op. cit., pp. 46, 55, 64, 65, 66, et seq.)

Hence it is a miserable play upon words to infer, that the *specific affection*, syphilis, *must also* have existed from time immemorial. It is to confound all affections of the organs of generation, with one which is the result of the application of a *specific* contagion.

A few of the advocates of the ancientness of this disease, have imagined that the miseries of Job were nothing more than the manifestations of the symptoms of secondary syphilis! A more striking and convincing illustration of the futility of all attempts at drawing definitive conclusions from such highly figurative and metaphorical descriptions, could not be afforded, than the fact that a modern physician, after the most patient and careful investigation of the medical literature of the Bible, concludes that the Hebrew poet was afflicted with *small-pox!* (Vide Medica Sacra, by Thomas Shapter, M. D.; London, 1834; as cited by Stewart, on the Diseases of Children. New York, 1841, p. 436.) Certain prohibitory and preventive statutes prescribed by the Jewish legislator, as recorded in Leviticus (xv. v. 2—23), have been conjectured to have reference to a contagious discharge from the urethra. Hippocrates speaks of suppuration of the genital organs, in connection with ulcerations in the mouth. Galen, in his Commentary, describes an *eating ulcer* of the genitals. Oribasius, Aëtius, Paulus Ægineta, and Actuarius, among the Greeks; Celsus, Pliny, and Cælius Aurelianus, among the Romans; Rhazes, Avicenna, Ali-Abbas, Avenzoar, and Albucasis, among the Arabians; all these describe, with more or less fullness, clearness, and sagacity, many derangements of the animal economy, which have been thought to bear a marvellous resemblance to syphilis. They speak of eating sores of the prepuce and glans; puriform discharges from the urethra; cancer and gangrene of the penis; enlargement of the inguinal glands; tumefaction of the testes; abscess, pustules, and gangrene of the vagina; fistulæ of the perineum and scrotum; strictures in the urethra; condylomatous tumors near the anus; warts on the penis and

labia, and several other local affections of a similar character. (Vide Dr. A. J. L. Jourdan, op. cit., p. 45, et seq.) The frequency of ulcers on the genitals, not arising from the poison of syphilis, is fully proved by the testimony of authors who wrote on the subject previous to the latter part of the fifteenth century, and who could not fall into the modern error of imputing every ulcer on the genitals to that source. Celsus describes eight species of ulcers to which the organs of generation are subject; three of which are at present as common as the syphilitic ulcer. (Vide Celsus, Edin. ed., 1809, p. 330 to 335.) The *bubo* was accurately described by Guillaume de Plaisance, who wrote in 1343. (Vide Ricord, op. cit., p. 29.) Now, I think it must be acknowledged by every unbiassed mind, that *not one, or in fact all of the symptoms above enumerated*, are very strikingly indicative of the disease which we *now* call syphilis. There may be some analogy to *gonorrhœal* affections in the diseases described by these authors, but assuredly none to those which are *genuinely syphilitic*. R. Carmichael asserts, that "there exists the strongest evidence, in the writings of all medical authors, about the year 1500, that syphilis was a new disorder, and baffled the skill of the most eminent physicians;" but very properly concedes, that "infectious ulcers on the organs of generation" prevailed, "long previous to any knowledge of syphilis." (Vide op. cit., pp. 22 and 30.)

But the partisans of its antiquity go further. They assert that Rhazes mentions an ulcer of the penis produced by the "*accensionem mulieris supra virum;* that sundry authors who flourished between 1270 and 1470, mention ulcers and pustules of the penis as contracted by *lying with foul women*. (Vide Druitt's Modern Surg., Am. ed. Philad., 1842, p. 189.) The following are a few of the authors who flourished at this epoch, and are cited by Astruc as containing passages bearing on the subject, viz., Gulielmus de Saliceto of Placenza, Lanfranc of Milan, Bernard Gordon of Montpelier, Guido de Cauliaco, Valescus de Taranta of Montpelier, and Peter de Argeleta of Bologna. But none of the affections treated of by these writers can be called syphilitic in the proper acceptation of that term. (R. Carmichael's Essay on Ven. Dis., by Emerson. Phil., 1825, p. 20.)

But it is quite manifest that such vague expressions cannot be considered sufficient to establish the validity of the claim to antiquity. The strongest and most vaunted arguments brought forward by the champions of antiquity, are contained in two papers presented by Mr. Beckett to the Royal Society in 1717 and 1718. (Phil. Trans., vols. 30 and 31.) The whole of his arguments are based upon the presumed *identity* of gonorrhœa and syphilis. He contends that as early as 1162, certain enactments were extant in England, providing against *the perilous infirmity of burning*. And, moreover, that a MS. in Lincoln College, Oxford, written by Thomas Gascoigne, Chancellor of that University, and dated 1430, states that some men had died of diseases caught by *frequenting women*. Pliny the younger relates an anecdote of a woman seeing her husband affected with ulcers on the genitals, and despairing of a cure, rushed with him into the lake Como. Josephus informs us that Apion died of an ulcer on his genitals. Herod, king of the Jews, according to the Hebrew author, met with the same fate. But neither Pliny nor Josephus ascribes these diseases to *intercourse with women*. The cruel monster, the Emperor Galerius Maximus, died of ulcerations of the genitals and fistulæ in perineo; the whole of his body exhaled a most offensive stench. (Vide

Druitt, op. cit., p. 190, and Jourdan, op. cit., p. 66.) But, under any circumstances, these historical cases may be considered doubtful; for the *lice* found between the hairs of the eye-lashes (the phthiriasis of the ancients), and below as well as above the cuticle in general, have been regarded as the reputed cause of the death of Plato, of Ennius, of Sylla, of *Herod*, and many other highly respectable men of antiquity. (Fletcher's Elem. Gen. Path., Edinb., 1842, p. 219.)

Allowing the supporters of the antiquity of syphilis the full force of the arguments drawn from the ancient authors, it would, notwithstanding, require the application of the most subtle dialectics of the antiquary to eliminate *such a disease* from these records. It is true that ulcers and pustules on the genital organs and sundry discharges, were not unknown; but still, "neither in Celsus, nor any other ancient writer, do we find mention that such maladies were *solely, or even frequently, the produce of sexual commerce;* or that they were peculiarly *difficult to heal;* or that they were frequently, or, indeed, ever, *followed by constitutional diseases.*" (Druitt, op. cit., p. 190). As an offset to balance the conclusions which have been deduced from the historical investigations of Mr. Beckett, I shall adduce the very *recent* researches of Professor Simpson, of Edinburgh. Professor Simpson has cited the official edicts of the Aberdeen magistrates, and of the King's privy council, respecting the new "infirmitey," issued during the year 1497, with various casual notices of the distemper from the contemporaneous writings of Dunbar, Lyndsay, Inglis, etc. Dr. S. alludes to an entry in 1502–3, in the privy-purse expense-book of Elizabeth, queen of Henry VII., as containing the earliest notice of the disease in *England.* The Aberdeen edict was dated about four years after the malady appeared in Italy, and is remarkable as ascribing the dissemination of it to impure sexual intercourse. Lastly, Professor Simpson proceeds to show, from the old data which he has adduced, that syphilis was *new* to this part of Europe at the date of the edicts; and that it was hence different, on the one hand, from *gonorrhœa*, and on the other, from the *leprosy* of the middle ages—both of which affections *were well known in Scotland before the era in question.* (Vide Lond. et Edin. Monthly Jour. Med. Science, as cited in New York Lancet, vol. i., p. 400. 1842). In short, even the most zealous defenders of the antiquity of syphilis have been compelled to admit that the disease which desolated Europe at the close of the fifteenth century, was, at least, a *new and virulent variety*, or modification, of the supposedly ancient malady.

In justice to the erudite Dr. A. J. L. Jourdan, it is proper to state in this connection, that he has deduced opposite conclusions with regard to the *antiquity of syphilis*, from the historical researches which he has made, and to which I have been so largely indebted; but his opinions accord with my own on every *other* point. I think, however, that a critical examination of the testimony which he has adduced on this head, shows that an inordinate zeal to support a favorite hypothesis has led him to confound *syphilis* with *other diseases* of the generative apparatus. At the conclusion of his essay, an acknowledgment is made, that the *contagious venereal affections* which were known at all times *among civilized nations*, are *very different from the epidemic of the fifteenth century.* (Op. cit., p. 107.) If the *syphilis* of the present day be identical with the above-mentioned epidemic, our deductions are, after all, not very discordant.

From what has been said on this point, I think we are justified in concluding this branch of our subject, by adopting the strong and emphatic language of Sir Charles Bell. He observes—"The Greek and Arabian physicians were accurate observers and good historians. Could they have failed to notice a disease characterized as this is,—death through stages so horrible? In the genitals, chancrous ulcers and *verrucæ*; buboes in the groin; wide-spreading deep sores; upon the skin, yellow and livid blotches; pyramidal scabs, and under them corroding ulcers; pustules and ulcerated tubercles around the temples and forehead; the bones of the palate lost; the lips, eye-lids, and nose swollen, distorted, and ulcerated; the hair fallen; the human features and the human voice lost; and the unfortunate victim racked with pains in the bones, succeeded by nodes and exostosis. Could a disease so horrid be overlooked? About the close of the fifteenth century the disease became universal. In a single life-time, there appeared a hundred books upon this disease, and not an allusion to it before." (Vide Dunglison's Am. Med. Library. Sir C. Bell's Institutes of Surg., p. 342. Philad., 1840.)

John de Vigo describes the chancre, or venereal pustule, as being "sometimes of a livid color, sometimes black, and sometimes whitish, *with a callosity surrounding it.*" This writer witnessed the *outbreak* of the disease, and the characteristic mark did not escape his observation. (R. Carmichael's Essay on Ven. Dis., by G. Emerson. Philad. 1825. p. 21).

2. *Was Syphilis imported from America?*

It is admitted, on all hands, that toward the end of the memorable fifteenth century, Europe was invaded by a disease which on all sides spread alarm, horror, and death. Marcellus Cumanus, who served with the Venetian troops under Charles VIII., in the campaign of 1495, was the earliest author who wrote concerning the history and cure of syphilis. (Vide Hennen's Principles of Military Surg. 1st Am. Edit. Philad., 1830, p. 384). A reflection that very naturally offers itself, when we meditate with impartiality on the history of syphilis, is, that it is certainly an unaccountable anomaly in the annals of human credulity, that the opprobrium of having originated such a disease, should have been so long fastened upon America. Indeed, the very idea, that a disorder arising from the grossest debauchery and obscenity, could have first developed itself among a people inhabiting the forest, and scarcely in the infancy of civilisation, is, in its very inception, *primâ facie* improbable. In some instances, however, erroneous opinions become so firmly rooted, that, for their eradication, the greatest struggles are necessary, and often will we find even the strongest efforts of reason to fail, against the power of habit, which always operates in their favor.

In the subjoined historical abstract, I have taken Dr. A. J. L. Jourdan as my guide, who has traced up the chronological details of this subject, with a degree of industry, accuracy, research, and erudition, which is absolutely astonishing. The first writers in Europe who described syphilis, were far from considering it of transatlantic origin.

At that period, the disease was attributed by many to a *celestial influx, or malignant conjunction of Saturn and Mars in the sign Scorpio*, or some other analogous astrological nonsense; by some, to *Divine vengeance*, some to an *earthquake*, and others to a *malignity of the air*, caused by an overflow of the Tiber; but *not one*, for the first twenty-five or thirty years,

ascribed to it an American origin. (Druitt, op. cit., p. 191.) Fracastorius ascribed it "to the different constitutions of heaven and stars, which but seldom happen, but may effect great matters when they do coincide." (Eberle's Prac. of Med., 4th ed., Phil., 1838, vol. ii., p. 503.) Matern Berlen, a clergyman of Ruffach, assigns it a very different origin; declaring it to have been a punishment inflicted by the Almighty on Charles VIII. for having carried off the Duchess Anne of Bretagne from the Emperor Maximilian, to whom she had been betrothed. (Dr. Good, op. cit., vol. ii., p. 91.) Leonhard Fioravanti, the Italian alchymist and empiric, propagated a still more singular fable on the origin of this affection. He says that during the famine caused by the siege of Naples,* the soldiers eat some articles prepared from *human flesh*, which soon affected them with syphilis. What appears most remarkable is, that Lord Bacon put faith in this absurd story, and even endeavored to render it still more plausible. Even later, in the year 1639, Peter Benoist, professor at Montpelier, maintained that syphilis owes its origin to anthropophagy. (Dr. Jourdan, op. cit., p. 9—10.)

In 1680, Samuel Janson, who had resided many years in the West Indies, without noticing syphilis to be there endemic, advanced the extraordinary hypothesis, that it was carried to the New World by the slaves from Africa! What is certainly very unaccountable, is, that it was embraced by Sydenham and E. Blancardus, and even enlisted the great Boerhaave, by whom it was defended with great warmth in 1751. This idea is irrevocably overthrown by the fact, that slaves were not brought from Africa to America previous to 1503; at which time the disease was prevailing all over Europe. (Jourdan, op. cit., p. 9.) Van Helmont broached the ridiculous idea that the disease had originated in America from the intercourse of the Indians with brutes! This was supported by Laurent Roberg, John Linder, and Heydentryk Overcamp. Joseph Xavier Bechman thought the Jews contracted it in Egypt during their captivity, and carried it to Palestine, whence it extended to the Christians during the crusades. This is in opposition to chronology, history, and reason. (Jourdan, op. cit., pp. 36 and 37.)

It is somewhat amusing to read the numerous hypotheses which have been broached by the syphilographists of the French school, to account for the manner in which the syphilitic virus is propagated. Thus, M. Bru declares that it is only the *mode* (leaven) which is inoculated, and that "the virus or pus is only the consequence of the neutralization" of the aforesaid *mode;* that the venereal action is the result of its combination with a substance "of a nature to set the phlogiston in action, since its first product is inflammation;" and that the afore-mentioned *mode* is "inoculated by virtue of a kind of electrification," "or some other modification of elementary fire altered and changed under a form of expansion." What this *mode* is, if there be such an entity,† is not rendered excessively clear, even by the luminous reasoning of M. Bru! M. Caron maintains "that the origin and contagion of the venereal virus are

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* Fioravanti has committed a historical blunder at this point; for Naples, at the time alluded to, was besieged neither by the French nor Spaniards. The French troops entered and left that city without encountering the slightest opposition.

† M. Ricord declares that it is "to be considered an entity in the same degree as the peculiar principle of hydrophobia, the venom of the viper, the specific cause of the small-pox," etc. (Op. cit., p. 30 et seq.)

a species of conception, and not the result of a simple intersusception, or absorption of a virulent fluid;" that it is "not a virus which is transmitted," but an "occult vice"—an inseparable "abstract essence." Next, M. Richond des Bru boldly affirms that every phenomenon can be explained without admitting such "a chimerical essence." (Vide M. Ricord's Treatise on Ven. Disease. New Trans. New York, 1842, p. 8 to 20.) Lastly, the researches of M. Donné have induced him to conclude, that the microscopic animalcule, the *vibrio lineola* of Müller, discovered by him in the matter of chancre and inoculated syphilitic ulcers, and those peculiar infusoria found in the pus of syphilitic vaginitis, and called *trichomonas vaginalis*, constitute the true contagious matter or principle. (Vide Ricord, op. cit. supra, p. 32 to 41.) As other microscopic observers, especially Henle and Gluge, have not found the *vibrio* in clean-kept chancres, and as they are absent in buboes and chancres from which M. Ricord produced syphilitic ulcers by inoculation, it cannot be regarded even as diagnostic of syphilis, and much less as the matter of contagion. The *trichomonas* has still less claim to be considered the contagious material. (Vide Fletcher's Elem. Gen. Path., Edin., 1842, p. 76.) This parasitical or entozootic hypothesis concerning the propagation of syphilis is not original with M. Donné. It was adopted by Abercomb, Hartsœcker, Calmet, and Deidier. M. Deidier asserts that it is demonstrated by Calmet, that the *lepra* of the ancients was produced by small living worms. He suggests whether the worms of the ancient lepra, which *were furnished with large teeth to gnaw flesh and stones*, might not, in the course of time, have changed their character, after the manner of the metamorphosis of the lepidopterous insects? "Or, perhaps," he says, "the worms of the lepra, which were gnawing insects, became aquatic from the development of their swimming membrane." By means of this hypothesis, M. Deidier explains why syphilis has become more tractable, and why mercurial remedies are so efficacious, since "all species of worms and ova are extirpated by mercury." (Vide M. Deidier's Dissert. Med. sur les Malades Vén., p. 13. Paris, 1770, as cited by M. Ricord, op. cit., p. 41.) According to Dr. Jourdan, this last opinion was advocated by A. Hauptman, Christian Lange, David Overcromby, Nicholas Hartsœcker, A. Calmet, A. Deidier, Herman, Athanasius Kircher, and Paul Juch. (Op. cit., p. 36.)

Such was the state of things, when in the year 1518, twenty-five years *after* the appearance of the disease in Italy, the opinion was suddenly advanced by Leonhard Schmauss, a German physician, that it had been brought from America. The year following (1519) the Chevalier Ulric Van Hulten, who afterwards became so celebrated for his zeal in the cause of Luther, his persecutions, and for the sarcasms thrown against him by Erasmus, and who, moreover, was tormented with the horrors of syphilis *for nine years*, adopted the opinion of Schmauss. His celebrity among the learned, and his elegance of style, contributed towards its diffusion. (Jourdan, op. cit., p. 7.) The opinion, however, would in all probability have sunk into merited oblivion, had it not pleased the Spaniard, Oviedo, in the years 1525 and 1535, to defend and propagate it, merely from motives of personal interest. The tone of assurance which he assumed, and his favorable situation, induced many persons to embrace his opinions; so that, in the space of fifty years, the American origin of syphilis was considered as one of the most authenticated and incontestible facts in history. (Jourdan, op. cit., p. 8.) Nicholas Massa, Leonhard Fuchs, J.

B. Montanus, Vidus Vidius, Gabriel Fallopius, J. Fernelius, Francis Renner, J. Crato Craftheim, Peter Lowe, Hercules de Sassonia, Eustachius Rudius, John Calvo, Andre de Leon, Fabricius Hildanus, Martin Lister, and many others, have been enumerated as the advocates of Oviedo's syphilitic romance. (Jourdan, op. cit., p. 8.) Nevertheless, in the midst of this general belief, some authors, and especially Van Helmont, discarded this preposterous notion. Finally, however, a man, whose merits have been greatly exaggerated, but whose vast learning and great patience in historical researches cannot be denied, undertook to defend the popular opinion upon philosophical grounds. I allude to Dr. J. Astruc. Convinced beforehand, that syphilis had been brought from America, and that it had not been propagated by the French, he wished all to be subservient to his views. He adopted implicitly the stories of Oviedo; he rejected as inaccurate and false, the observations of those who contradicted him; from garbled extracts, he drew erroneous conclusions, and he even did not scruple to commit a number of chronological errors, much less pardonable in him than any other individual. His design succeeded, and his historical romance on syphilis was universally applauded, and soon boasted as many defenders as readers. Haller, Van Swieten, Robertson, Cullen, Turnbull, Bertrandi, and a host of others, adopted and supported it. Less partial observers, however, among whom were Antony Nunnez Ribeiro Sanchez, Philip Gabriel Hensler, Kurt Sprengel, and ultimately Christian G. Gruner, far from being dazzled by the specious reasonings of Astruc, or borne down by his torrent of advocates, criticised, commented on, and refuted the work of their celebrated predecessor. During the controversy, Christopher Girtanner—the most recent defender of the American origin of syphilis—in the absence of valid argument, had recourse to the most violent invectives. (Jourdan, op. cit., p. 10—15). Such is a brief and imperfect historical sketch of the opinions entertained with regard to the origin of this disease.

From the unanimous silence of those writers who were eye-witnesses of the first outbreak of syphilis, not one of whom ascribed to it an American origin; from the fact "that neither Columbus himself, nor his brother, who left such accurate narratives of his voyage," makes the least mention of such a disease having been discovered among the natives, or prevailing among the crews of their vessels, which, moreover, must have incapacitated them for every kind of exertion; from these and other considerations, it might be presumptively inferred, that, the supposition of syphilis being imported from the New to the Old World, through the followers of Columbus, is exceedingly *improbable* and very repugnant to reason. (Thomson, Medico-Chirurg. Rev., Dec., 1821, p. 617.) By an analogous process of reasoning, Dr. W. Poyntell Johnson, of Philadelphia, justly concludes, that "the amount of probabilities is in absolute opposition to its American origin." (Vide Med. Examiner, Philad., 1840, vol. 3, p. 789.) But I go further, and maintain that history and chronology alike demonstrate the *impossibility* of this origin. Before entering into any details, it is indispensable to ascertain accurately the precise time at which syphilis manifested itself in Europe. To do this, it is necessary for me to anticipate much which ought properly to have come under my *third* division of the subject.

From a careful analysis of the writings of contemporaneous authors, the following deductions have been drawn. According to Massa, J.

Cataneo, Peter Pinctor, J. Burchardi, and E. Capreoli,* syphilis already existed at Rome and in Italy, in 1494; according to N. Leoniceno B. Fulgosi, Mark A. Sabellico, Stephen Infessura, and P. Delphini, at Rome and in Upper Italy, in 1493; finally, according to Fulgosi, in Upper Italy, in the year 1492. Infessura's testimony is positive, and has never been called in question; and he fixes on the *month of June of the year* 1493, as the epoch at which this disease manifested itself at Rome. (Vide Jourdan, op. cit., p. 19, et seq.) Christopher Columbus, on his first return to Europe, arrived at Valparaiso, near Lisbon, on the 4th of March, 1493. He remained nine days with John II., king of Portugal; he again set sail and reached Seville on the 15th of the same month; and at last arrived by land at Barcelona about the middle of April. (Jourdan, op. cit., p. 24.) Girtanner maintains, on the authority of the assertion of Oviedo, that syphilis was carried to Italy by the troops of Gonzalvo of Cordova.

Girtanner places great reliance on the testimony of this historian, from the circumstance that he was in Spain when Columbus returned, and went himself several times to the West Indies, and there filled the most important offices. In view of a few historical facts, this evidence is greatly weakened. Gonzalvo Ferdinand Oviedo was born in 1478. Consequently, *he was only fifteen years old*, when Columbus returned from his first voyage in 1493. *He was not sent to St. Domingo until* 1513; that is, *until twenty years after syphilis had broken out in Italy.* His history of the West Indies was *first* published about the year 1525. (Dr. Jourdan, op. cit., p. 35.) Can his testimony balance that of contemporaneous physicians, whose judgments had been matured by age, study, and experience?

Now, we have already proved from the most accurate chronological researches, laying aside all authorities of a dubious nature, that the disease was raging at Rome and in Italy, about the month of June, 1493. This incontestible fact, by itself, excludes the *possibility* of its having been carried there by the army of Gonzalvo, as this general *arrived in Calabria as late as the month of May*, 1495. A part then, of the romance of Astruc and Girtanner, is, by itself, overthrown; for the disease had existed in Italy *two years* before that time. (Jourdan, op. cit., pp. 24, 25. Also, Dr. Good, op. cit., vol. 2, p. 91.)

Next, let us examine whether syphilis really existed in the West Indies and on the continent of America, at the time the Spaniards arrived there. This is the more necessary, as from the 4th of March, 1493, the period of the first landing of Columbus, to the month of June of the same year, there is an interval of three months, during which the supercilious caviller might insist, though with little probable semblance of truth, that the disease had been propagated from Spain to Italy. Girtanner cites the following authorities to prove that syphilis was endemic on the continent of America, especially in Peru and the Floridas, viz., Lopez de Gomara, Andrew de Leon, Augustin de Zaratte, and Jerome Benzoni. He seems

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* Gaspard de los Reies Franco, a Spanish physician, has collected several passages from these authors, showing that the disease was known in Italy anterior to March, 1493. Peter Pinctor says that it was raging at Rome in March, 1493. (Jourdan, op. cit., p. 23.) Nicholas Leoniceno says that it appeared in the memorable year of inundation; viz., according to Infessura and Benedetti, in October, 1493. (Ibid., p. 22.)

to have forgotten that Peru was discovered as late as 1531, or thirty-nine years after the disease had been known in Europe. The same is true of the Floridas, "which were discovered by Cabot, seen afterwards, in 1512, by Ponce de Leon, by Cartier in 1535, by Ribaut in 1562, and by Landonnier, in 1564." Besides, Gomara is guilty of a gross and palpable contradiction when he asserts that the malady is a *new one*, and yet says it is *syphilis!* (Jourdan, op. cit., p. 32.)

By tracing in succession the authorities which have been adduced in support of this opinion, weighing their respective titles, and marking with attention the degree of credit which they deserve, we shall behold the already tottering edifice of the American origin of syphilis, give way even to its very foundations. Oviedo is, as usual, the great authority on this point.* But the testimony of such a man is worth less than nothing, when it is considered, that Ferdinando Columbus, A. Herrera, De la Casa, and other *contemporaries*, accuse and *convict* him of the grossest falsehoods, contradictions, and inaccuracies. (Jourdan, op. cit., p. 27.) In fact, this tyrant had always made bad use of his power while in the West Indies, and was glad to report anything which represented the unfortunate people as wicked and dissolute, as a means of justifying his conduct. There is, consequently, no evidence of the disease having existed in America until the *third* voyage of Columbus, in 1498, when it was probably carried to St. Domingo by his crews; and therefore, it was not before it had *prevailed in Europe for five years.* (Jourdan, op. cit., pp. 27 and 29.)

From the foregoing remarks it appears, that from 1518 to 1546, the American origin of syphilis was supported *solely* by the faith of Schmauss and Van Hulten, and the unproved and uncertain accounts of Oviedo; but that it was ultimately rendered a medical dogma by Astruc. The principal arguments of Schmauss, Hulten, and Oviedo, are founded on the fact, that *guaiacum*† is indigenous to America; although it was not brought to Spain until 1508. They urge the absurd and ridiculous notion that nature always provides an *antidote* in the vicinity of a poison! (Jourdan, op. cit., pp. 26 and 34. Also, Encyc. Am., Philad., 1835. Art. Syphilis.) But besides, Peter Martyr, who was physician to Ferdinand and Isabella, and was actually at Barcelona when Columbus returned from his first voyage in 1493, does not say a word as to its American origin. (Druitt, op. cit., p. 191.) Indeed, even the advocates of the American origin of syphilis, have been compelled to abandon the argument grounded on the *first* voyage of Columbus, and to fall back upon his *second* return, that is, in 1496; but this period is too late, as we have already traced its existence in Italy in June, 1493. It has, therefore, been proved, beyond the possibility of a doubt, that syphilis was not derived from America.

The advocates of the American origin of syphilis, cite the writings of

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* Francisco Delgado, who wrote in 1529, Alysius Lobera, Andrew Vesalius, Jerome Cardan, A. Cesalpinus, and even in 1546, Jerome Fracastorius, the intimate friend of Oviedo, discarded the opinion of its transatlantic origin. (Jourdan, op. cit., p. 34).

† Guaiacum, a remedy once so much exto'led in the treatment of syphilis, as to have acquired the appellation of *lignum sanctum*, was first introduced into Spain by Gonsalvo Ferrand, in 1508, who went to St. Domingo, and was cured of a long standing venereal complaint under the use of it. At one time it sold for seven gold crowns per pound! (Pereira's Elem Mat. et Therap., 2d Ed. Lond, 1842, p. 1653.)

Diaz Je Isla, Ferdinando Columbus, the son of the Admiral, Gomara, A. Herrera, and the celebrated Francis Guicciardini, as authorities on this question. According to Welsch, Diaz Je Isla's work was not written until 1555. He could not, therefore, have been an ocular witness. Columbus does not mention that the disease affected the colony at St. Domingo, *until the third return of his father in* 1498. He notices in his narrative that the *natives* were subject to a *cutaneous affection*, called by them *caracaracol;* but there is no reason to suppose that it was *syphilis*. Gomara wrote too late, having in fact copied the opinion of Oviedo. The same objection applies to Herrera and Guicciardini; the *former* wrote in 1600, and the *latter* was *only eleven years old* when syphilis invaded Europe. They both borrow incessantly from Oviedo. (Dr. Jourdan, op. cit., p. 28 to 32, and seq.)

In short, the whole purport of the vigorous attempt which was made in Europe, to fasten this opprobrium upon the New World, seems to be nothing more than a malignant echo of the absurd and ridiculous calumny, at one period so extensively credited, that the human species, and indeed the whole animal kingdom, had become entirely *degenerated* in America!

3. *When and where did Syphilis originate?*

After what I have already remarked under the *second* head of our subject, it will be unnecessary to adduce many observations on this point. Some authors maintain that the disease broke out several weeks *before* Columbus landed in March, 1493; that on his reaching Seville it had spread over Auvergne, Lombardy, and various other parts of Italy; and that, in the course of the ensuing summer, it was observed in Saxony, Brandenburg, Brunswick, Mechlenburg, and especially Strasburg, as many German writers concur in admitting; and even at Cracow in Poland, according to some. (Dr. Good, op. cit., p. 91.) We have already shown that there exists *positive* historical evidence that syphilis was raging at Rome in June, 1493;* but how long *before* this epoch it prevailed in Italy, it is hardly possible to determine with absolute certainty. The severe edict of the parliament of Paris, dated March 6th, 1496, proves that it had been raging in that capital *two years;* and that consequently it was known *there* in 1494; yet the conquest of Naples by Charles VIII. was not effected till February, 1495! (Jourdan, op. cit., p. 18 and 35. Also, Cooper's Surg. Dict., N. Y., 1832. Art. Venereal Disease.) Still, it is alleged that his soldiers took the disease from the followers of Columbus! The edict published by James IV., on September 22d, 1497, shows that it existed at Edinburgh at that date. (Vide supra; also, Jourdan, op. cit., p. 35.)

Dr. Parr supposes that the origin of the disease is more connected with the expulsion of the Moors from Spain. (Med. Dic., Philad., 1819. Art. Lues Venerea.) Dr. Jourdan deduces the same conclusion from his laborious researches. He observes,—"When we compare the testimonies of the most veridical historians and physicians, we think it

* During its first ravages, syphilis seemed to attack the wealthy in preference, as did the *mentagra* described by Pliny. It is well known that it spared not one member of the family of Alexander VI, the incestuous father of the equally incestuous Cæsar Borgia. In consequence of this, James de Bethencourt called it *morbus magnatum*. (Vide Jourdan, op. cit., p. 103.)

impossible to doubt its being derived from the *Marranes*" (an opprobrious epithet, signifying "hog," applied to the Jews and Moors), "who were expelled from Spain before the discovery of America." (Op. cit., p. 99.) These unfortunate wretches, who left Spain in the year 1492, to the number of 170,000 families, retired, for the most part, to the northern shores of Africa, where 30,000 of them were destroyed by a peculiar and most fatal epidemic. Many of them went to Italy, and encamped near the Appian gate at Rome, living in the most disgusting filth, and in total privation of all wholesome food. Infessura, who first notices the ravages of syphilis at Rome, calls it *pestis Marranorum*. Beniveni, Benedetti, and Fracastorius derive it from Spain. Leo, the African, declares that the disease was unknown in Africa anterior to the landing of the *Marranes*. (Jourdan, op. cit., pp. 99 and 100.) At all events, the origin of syphilis, and the expulsion of these victims of avarice and intolerance, were coetaneous events; but whether they stand in the relation of cause and effect, will, probably, always remain undecided.

Before concluding, I may be permitted to remark that, with regard to that horrible modification of syphilis, which scourged a great part of Europe during the close of the fifteenth century, it has been supposed that it is necessary to assume the existence of some other mode of propagation besides coition, to account for the extraordinary rapidity of its extension. The virulence of the disease, at the close of the fifteenth century, appears to have been really frightful, to judge from the descriptions of contemporary authors. Thus a German author of 1496, says: "It was a disease sent down from the citadel of the immortal gods on the French—a most horrid and terrible prodigy, unheard of, hated, and unknown—a disease repugnant to nature." (F. C. Skey's Lec. on Ven. Dis., Med. Intelligencer, vol. iii., p. 102. Philad., 1840.) Consequently, it has been supposed to have prevailed *epidemically* at that early period of its history; or else, in accordance with the observations of the writers of that age, that it was communicated from one person to another merely by the touch, wearing infected garments, etc. In truth, unless some cause of an analogous character be admitted, the propagation of the disorder throughout the continent of Europe in the short space of *two years*, would imply, or presuppose, an extent of licentiousness and a refinement of depravity, beyond the utmost bounds of credibility.

PART SECOND.

Critical Analysis.

ART. IX.—*Communication from the Board of Health of the City of New York, in Reply to certain Interrogatories relative to Yellow Fever, made by the Board of Health of New Orleans.* 1843.

Researches in Medicine and Medical Jurisprudence. By JOHN B. BECK, M. D., Professor of Materia Medica and Medical Jurisprudence, etc. 1835. Article—"*On the Non-Contagiousness of Yellow Fever.*"

An Account of the Epidemic Fever which prevailed in the City of New York, during part of the summer and fall of 1795. By RICHARD BAYLEY. 8vo. pp. 160.

The Medical Works of EDWARD MILLER, M. D., Prof. of the Practice of Physic, etc. Article—"*Report on the Malignant Disease which prevailed in the City of New York, in the autumn of* 1805; addressed to the Governor of the State of New York."

Remarks on the Yellow Fever of New York in 1741 *and* 1742. By the Hon. CADWALLADER COLDEN. Written in 1743. Communicated by his grandson to the Medical Repository, third Hexade, Vol. II.

Extract from an unpublished Work on the Importation and Propagation of Plague and other Contagious Diseases. By Dr. FERGUSSON, Inspector-General of Army Hospitals. Article—"*Yellow Fever.*" Vide Edinburgh Medical and Surgical Journal, for July, 1843.

THERE is scarcely any medical subject of greater interest to the inhabitants of our Atlantic cities than that of the Yellow Fever; for, independent of the many valuable lives thus lost to the community, our commercial prosperity, when this disease prevails, or is reputed to prevail, is not only embarrassed at home, but also abroad by long and expensive quarantines. Under this view of the question, we conceive it to be our duty to make a permanent record of the subjoined paper, which, as an official document, is the only one that has appeared since 1822, with the exception of those of the present year. The following paper will explain itself:

"CITY OF NEW YORK,
Board of Health, August 27, 1842.

"The following communication was presented by the President from the Board of Health of New Orleans, and referred to a Select Committee.

"NEW ORLEANS, 15th August, 1842.

"Sir—We have the honor to inform you that, at a recent meeting of the New Orleans Board of Health, the undersigned were appointed a committee to col-

lect and report such facts as may tend to elucidate the question, whether Yellow Fever is to be considered of Domestic or Foreign origin, so far as regards the United States of America, and especially as relates to this city.

"Your city, we hear, has occasionally been visited by this disease. Therefore we take the liberty of applying to you for such information as may have led you to regard Yellow Fever either of Domestic or Foreign origin, and the advantages, if any, resulting from the strict enforcement of quarantine laws in preventing an invasion of this formidable malady.

"We are aware that we are asking at your hands the performance of a labor of no small magnitude. The apology we would offer for thus trespassing upon your time, is the deep interest, involving the lives of a large portion of the human family, found in the questions respectfully submitted to you. With high consideration and regard, your obedient servants, C. A. Luzenberg, M. D.,
J. Rhodes, M. D.

"To the President of the New York Board of Health."

"Board of Health, January 4, 1843.

"The Select Committee, to whom was referred the letter from the Board of Health of New Orleans, presented the following communication thereon, which was accepted, and directed to be transmitted to the Board of Health of New Orleans, and published in the public newspapers. Jos. R. Taylor, Clerk.

"New York, November 25, 1842.

"To the Committee of the Board of Health of New Orleans—C. A Luzenberg, M. D., J. Rhodes, M. D.

"Gentlemen—The communication which you did us the honor to make to the Board of Health of the City of New York, came duly to hand, and was referred to a special committee of the Board to report thereon. The undersigned having with all due deference considered the same, and concurring with your committee in the importance of the interrogatories embraced therein, take this, the earliest opportunity at their command, to give such views on the subject of your inquiries as may the better enable your Honorable Body to act in the premises, aware that every aid ought to be imparted to competent inquirers, in order the better to carry out improved legislation on that grievous pestilence which has so often afflicted both New York and New Orleans, as well as other prominent cities in the Union.

"The numerous records which have been made of the occurrence of yellow fever in the city of New York, more especially since the year 1791, up to its last invasion in 1822, have led the most cautious observers of the disease to look upon yellow fever as a disorder *sui generis*—a type or form of fever distinct from the ordinary bilious and intermittent fevers which occur among us; that it is characterized by a train of symptoms in no wise designating the febrile complaints which seem to be more especially the native products of our country: and these opinions of yellow fever are strengthened by the fact, that it demands a method of treatment which varies in many of its essential points from that which is found effective in that large class of disorders which are commonly grouped as the products of marshy emanations, of vicissitudes of weather, or of typhoid exhalations occurring in jails, prisons, and the like. The whole history of the yellow fever pestilence, from its invasion, or first attack, to its too often fatal termination, demonstrates this important therapeutical doctrine.

"Notwithstanding the force of this truth, the origin of the yellow fever among us has been the subject of much controversy, and long and acrimonious disquisition; many contending that the disease is a domestic production, arising from circumstances inherent in our climate, and depending for its creation upon peculiar combinations of local causes; and, indeed, it has been affirmed that our climate (that of New York) is the actual latitude of the pestilence, and that we need not look elsewhere for its birth. This view of the domestic origin of yellow fever has been sustained by many of our most eminent writers.

"On the other hand, there is and has always been among us, a body of practical observers, who, having encountered the disease in its most malignant forms,

when it has assumed an unusual degree of virulence, as occurred in the years 1795, 1798, 1799, 1803, 1805, and in 1822, have strenuously contended in behalf of its exclusively foreign origin; that it was a disorder of a type essentially different from the febrile affections which are endemical to our country, and that its introduction among us has in all cases been satisfactorily demonstrated to have been from abroad, conveyed hither by shipping arriving from sickly ports, or sprung from vessels coming from distant parts and in a foul and pestilential condition. The greater immunity of the human constitution from a second attack of yellow fever seems also to give additional countenance to the theory of its more distinctive character.

"These two different and opposite opinions of the essential attributes of yellow fever have been maintained with all the bitterness of controversy, and much has been published on both sides by the respective contending parties. It would have afforded great satisfaction to the Committee to have laid before your honorable body the leading documents which have been printed on the subject; but they are not at this time within the command of the Committee. This circumstance is perhaps to be the less regretted, inasmuch as the New York Board of Health have ever exercised, amid all the severe trials to which they have been subjected, a strong, determined, and impartial judgment in the premises; listening with all due regard to the various representations made to them by the several parties or advocates, and exercising the powers vested in them by statutory law, so far as their capabilities enabled them, to alleviate the calamities incident to the existence of the pestilence when it prevailed, and removing every cause which seemed in any wise to aggravate the evil or give wings to the disorder. The undersigned believe they speak a language in uniformity with that which might be uttered by our citizens at large, that the Board of Health of New York have, on every recurrence of the yellow fever in the city, profited by the experience of those who have preceded them in the discharge of similar trusts in like emergencies in the metropolis; and the conviction is general and freely acknowledged, that at the present day, the salutary regulations which are adopted, as respects the improved condition of our medical police and our quarantine establishment, have proved a source of incalculable benefit to New York, in the saving of innumerable lives and in protecting those interests so dear to her as a mercantile emporium.

"Your Committee solicit from our Board, in an especial manner, to make known to your Honorable Board, 'the advantages, if any, arising from the strict enforcement of quarantine laws in preventing an invasion of the formidable malady.' We are ready to give answer to your important interrogatory in terms the most unequivocal, and we hope satisfactory. Our present quarantine regulations are the result of much experience and of repeated emendations. A copy of them we have ordered to accompany this letter; they will be the better understood when thus submitted to your full consideration. We have cogent reason to believe, that from defects in former statutes on this important subject, and, perhaps, from occasional relaxations in their enforcement, we have in some seasons paid a severe penalty. Since the rigid adoption of our improved code, we have congratulated ourselves upon our freedom from the pestilence; and the confidence we enjoy is strengthened in the contemplation that yellow fever is now known to the junior members of the medical profession only by name; our city having been exempted from this direful scourge for the unprecedented period of twenty years.

"Before we conclude this official communication to your Honorable Board, we feel obligated to state, as a further evidence of the value of our quarantine laws, as now enforced, that cases of yellow fever are almost every season brought up to our quarantine establishment, at Staten Island, and there subjected to the medical discipline of the medical officers of that institution. Yet by the regulations of our sanitary code, the city itself has entirely escaped the annoyance of even a sporadic case. Of the nature of these cases of yellow fever, which are at some seasons brought to the quarantine establishment, we may add the fact that instances of the extension of these cases to the attendants at the quarantine, have occasionally occurred; thus presenting still stronger testimony of the vast

importance of our restrictive measures. No one could predict the evils which might flow from such cases being permitted to enter our city by shipping or other means.

"All which is respectfully submitted:

JOHN W. FRANCIS, M. D.,
Resident Physician.
A. SIDNEY DOANE, A. M., M. D.,
Health Officer of Port of New York.
WM. TURNER, M. D.,
Health Commissioner.
ROBERT H. MORRIS,
MOSES G. LEONARD,
CHARLES P. BROWN."

Although this document is open to several grave objections, yet we shall pass them over, inasmuch as our object is mainly to put it here on record as supplying a link in the historical chain of yellow fever in this city.

We shall also avail ourselves of the opportunity now presented, to make a correction in reference to Prof. John B. Beck, whom we innocently misrepresented in our last number, by a quotation from the "Cyclopædia of Practical Medicine," in which it was said that, in 1823, he made a public retraction of faith as to the contagion of yellow fever—a belief which, as appears from his "Researches in Medicine and Medical Jurisprudence," he had never entertained and never avowed.

"It is in consequence of such representations," he says, "that the present paper has been looked upon by some eminent individuals, as a recantation of previous faith.* Proud as the author would be of the implied magnanimity of publicly retracting what observation and reason had shown him to be erroneous, he is not justly entitled to the compliment. That his prepossessions may originally have been in favor of the contagious character of yellow fever, he is not prepared to deny. Educated in a school in which this was the prevalent doctrine, how indeed could it be otherwise! He is not aware, however, of having ever committed himself on this subject, and the whole amount of his recantation will appear from the paper itself. As an inquirer after truth, he refrained from forming any definite opinion on so important a subject, until he should have had the opportunity of personal observation. This opportunity occurred partially in 1819, when the fever prevailed in this city to a limited extent, but more particularly in 1822, when it prevailed more extensively, and furnished the amplest materials for making up a rational judgment."

In this paper, Prof. Beck confines himself exclusively to the question of the contagious, or non-contagious, character of yellow fever; and to be enabled to appreciate the force of his argument, the following brief history of the epidemic of 1822 becomes necessary:

"The disease made its first appearance on the 10th of July, 1822, at the lower part of Rector-street, a small street opening upon the Hudson river. Three cases occurred on the same day, two of them at the house situated on one

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* De l'opinion des Medecins Americains sur la contagion ou la non-contagion de la Fievre Jaune, etc. Par N. Chervin, M. D., &c., p. 11. 8vo. Paris, 1829.

See also an able Essay on Yellow Fever, by I. Gillkrest, M. D., Deputy Inspector General of the Army. From the Cyclopædia of Practical Medicine, p. 30. London, 1832.

corner of the street, and one at the opposite house. In a few days several cases occurred at a neighboring house in the same street, and from these points it continued gradually to extend itself in all directions. About the 1st of November, when the disease terminated, the cause of the disease was prevalent over the whole of the lower parts of the city, extending northward as far as Chambers-street. The portion of the city thus affected, was denominated and currently known by the title of the 'infected district.' As the poison spread itself, the citizens removed before it, and the limits of the district were marked by barring up the streets. The spread of the disease was announced by the occurrence of cases just beyond these limits, and when these occurred, the barriers were extended correspondingly. The sick were in all cases removed from the infected parts, to other parts either of the city or of the surrounding country. The whole amount of cases that occurred, was about four hundred, of which about two hundred and thirty terminated fatally."

Having had much personal experience of the disease in 1822, which led to a positive and complete conviction that contagion had nothing to do in the propagation of it, Dr. Beck presented his views in a paper remarkable for the appositeness of its facts and the cogency of its reasoning.

"The believers," he says, "in the doctrine of the contagious character of yellow fever, may be divided into two classes. 1. Those who contend that the disease is contagious under all circumstances, and that it may as readily display this character in the pure and healthy air of the country, as it does in the confined air of our cities. 2. Those who admit that in the pure air of the country it is seldom if ever contagious, while in the impure and vitiated atmosphere of our cities it proves extensively so. Both classes, however, agree in this; that every case of the fever which occurs, must result from contagion, as they do not admit the existence of any other cause capable of producing the disease.

"As we wish to meet both these views of contagion, the following facts shall be arranged under two heads: Those which prove that the yellow fever of 1822 was not contagious in the pure air of the country: Those which prove that it was not contagious in the vitiated and impure air of our city.

"1. *Facts and Arguments which prove that the Yellow Fever of* 1822 *was not contagious in the pure air of the country.*

"(*a*) From the official returns of Dr. Joseph Bayley, Health Officer of the port, it appears that during the prevalence of the yellow fever in this city during 1822, seventy persons sick with that disease, were sent down to the Marine Hospital,* on Staten Island. Of this number, thirty-seven died, eighteen of whom had black vomit. The first of these patients was received into the hospital on the 13th of August, and died on the 15th. From that period until the final cessation of the fever, patients were almost daily carried down to this place.† These cases were regularly attended by the physicians and nurses of the establishment, not one of whom became affected with the disease; nor has a single case come to our knowledge of any person taking it, who was engaged in transporting the sick from the city to the hospital."

Our space will not allow us to follow Prof. Beck in the abundant

* Situated at the Quarantine establishment, about six miles below the city of New York, on the west side of the bay.

† A history of the proceedings of the Board of Health of the city of New York in the summer and fall of 1822; together with an account of the rise and progress of the Yellow Fever, which appeared during that season, and the several documents in relation to it, which were laid before the Board. Published by order of the Board of Health. New York, 1823, p. 132.

details which he gives in support of this position; nor can we follow him in a similar investigation, establishing the same conclusion relative to the spacious dwelling-house at Kip's Bay, provided at the public expense, for the reception of such of the inhabitants from the infected district as were unable themselves to provide a refuge; nor lastly, can we do more than merely advert to the fact that among thirty-six cases of the disease contracted in the infected district, but which were not developed until after their removal into distant and different parts of the country, and of which no less than twenty-seven cases proved fatal, there was not a single instance in which the disease was communicated. Let it, therefore, suffice to give Prof. Beck's general conclusion:

"Let us now," he says, "pause and reflect upon the amount of testimony already advanced. It appears then, from the foregoing facts and statements, that upwards of one hundred persons, sick of yellow fever (about one-fourth of all the cases), lay in different parts of the country, without, in a single instance, communicating the disease. If it had been previously proposed to settle this question by actual experiments, we cannot conceive that any could have been suggested which would have been more satisfactory to all parties, than the very ones which actually transpired during the summer and autumn of 1822. That one hundred persons, sick of a disease highly malignant and contagious, located in different parts of the country, should not, even in a solitary instance, have communicated it to second persons, seems to us literally impossible; and we believe, that were it not for the influence of preconceived opinions and long-cherished theories, facts of this sort would come home to the mind of every man with a force perfectly irresistible. Aware of these facts, the contagionists, so far from abandoning their favorite theory, have invented new schemes for upholding it. One of the most popular of these at present is this, that Yellow Fever, although it may not prove contagious in the pure air of the country, yet is eminently so in the impure atmosphere of cities, &c. Whether any countenance was given to this doctrine, by the facts which were developed during the fever of 1822, we now propose to investigate."

In the second place, Professor Beck adduces facts and arguments which prove that the yellow fever of 1822 was not contagious in the impure air of the city; and he is not satisfied to expose the error of the affirmative of this doctrine by proving that it is not correct in fact, but proceeds to show that it is unphilosophical in principle.

"If contagious effluvia emanate from the body of a person sick of a peculiar disease, they can be influenced by the surrounding air only in one of two ways. First, the surrounding air may serve simply as a medium of transmission. In this way the effluvia are diffused more widely than they would be in vacuo. It is evident, however, that here the contagion remains unchanged in its character, and produces its specific effects without any aid from the atmosphere, except that of enabling it to act at a greater distance from the sick body. Second, the contagious effluvia may enter into chemical combination with the surrounding atmosphere. An entirely new compound then is formed, the effects of which upon the human system must necessarily be different from those of the original contagion.

"These are the only possible methods in which contagious effluvia can be influenced by the surrounding atmosphere. Now, if we apply these propositions, and push them to their conclusions, they will be found to destroy completely the theory of which we are speaking. If the contagious effluvia enter into chemical union with the air, and form a new poison, then the same specific disease cannot be reproduced: this supposition, therefore, is inadmissible. If, on the other hand, the air serves merely as a medium for transmitting the poison to a greater

distance, then no reason can be assigned why, if you approach near enough to the sick body, contagion should not display itself in a pure as well as an impure atmosphere.

"Let us take another view of the subject. If a disease be contagious in one kind of air, and not in another, then it must acquire its contagious character from some peculiarity in the air in which it is so; and if this be the case, then the principle of contagion must exist in the air, and no reason can be assigned why the air itself should not, under these circumstances, produce the disease, independently of all sick and diseased bodies. This seems to us to be an inevitable conclusion from the premises; and to our minds, it appears most satisfactorily to do away the necessity of resorting to contagion to account for the origin and propagation of yellow fever. On these grounds, we think the popular doctrine which supposes that this disease may be contagious in one species of atmosphere, and not in another, is unphilosophical."

Not, however, content with abstract reasoning alone, Professor Beck again appeals to facts. He first refers to the *uninfected* parts of the city, showing that among upwards of a hundred persons lying sick of yellow fever beyond the limits of the infected district, not a single instance was known in which the disease was communicated to a second person. So notorious was this fact, that even those most zealous on the side of contagion, were forced to acknowledge its truth.

We now come to the question, whether the disease was contagious in the *infected* portion of the city.

"It is here, after all, that he makes his principle stand in defence of contagion. It is here, in fact, that all the contagionists are obliged to take refuge, or abandon their doctrine altogether. Let us see whether it can be defended with more success here, than in other places. For the purpose of simplifying the subject, we shall first state the question at issue. The contagionists assert, that after the first cases of the disease at Rector-street, every subsequent case was the result of some communication with those already sick; and that, as cases multiplied, the air itself became so impregnated by contagious exhalations from sick bodies, as of itself to produce the disease in those exposed to it, without immediate or direct connection with the sick. The non-contagionists, on the other hand, assert that the disease did not spread in consequence of any communication with the sick, and that the infection of the district was owing to other causes than exhalations from those sick of the fever.

"From this statement, it appears that both agree in one thing, viz., that the air in a certain portion of the city, called the 'infected district,' was actually impregnated with a certain poison capable of producing yellow fever in those exposed to it, without their having any immediate communication with the sick. This being agreed upon, the only remaining question to be solved is this: was the poison, so impregnating the air, and producing such effects, an exhalation from the bodies of those who were sick with the fever, or was it not? If it was, then the contagious character of the disease is established. If it was not, then the disease cannot be considered as contagious.

"Our reasons for believing that the contamination of the air was not owing to any exhalations from the sick, are the following:

"1. It has already been shown, that not merely in the pure air of the country, but even in the most impure and unhealthy parts of our city, patients sick of the yellow fever, in 1822, were uniformly approached with perfect impunity. The *air*, therefore, in the infected district, must have been much more venomous than the contagious poison itself, coming off directly from diseased bodies. That is, the poison diluted in atmospheric air, must have been more powerful than the pure, unmixed poison itself! A proposition, absurd in itself, and contrary to all analogy. We infer, therefore, that as the *air* of the infected district was more deleterious than actual contact with the sick, the poison existing in the air must have been some other than effluvia from the bodies of the sick.

"2. If the infection of the air depended upon emanations from the sick, then it should have extended *pari passu* with the sick and the dead. That this was not the case is notorious. That it was not so out of the infected district, is proved by the fact already established, that no case of communication was known; and that it was not so in the infected district, is equally evident from the circumstance, that the limits of the district were only known and defined from persons being taken unexpectedly sick, without, in many instances, any sort of intercourse with those already affected.

"3. The gradual and measured extension of the limits of infection proves conclusively that it must have originated from some other cause than contagion. So regular was this extension, that attempts were made, and with apparent success, to calculate its daily progress. From the point at which it first commenced, it extended very nearly to equal distances in all directions. Can any man, of common reflection, contend that such an effect could follow from patients scattered in different directions?

"4. But what settles this beyond peradventure, is the fact, that by far the largest number of the sick did not lie in the infected district at all. From the official list published by the Board of Health, it appears that in all, not more than about one hundred and thirty lay sick in the infected district during the whole of the prevalence of the fever, while about one hundred and seventy-two lay sick in different parts of the rest of the city. As, therefore, one hundred and seventy-two patients did not infect the air in which they lay sick, it is reasonable to conclude, that a *less* number would not produce such an effect; and, therefore, that the infection of the district was not owing to the sick.

"From all these reasons, we conclude, that the infection of the air was not owing to contagious exhalations from the sick, but to some other cause. What this cause was, it is not our business at present to inquire. That it was a gaseous poison, is evident—that it was permanent in its action is also clear from its gradual extension until the appearance of frost. Whence or how it originated, is a question very far from being settled, the investigation of which we may, perhaps, take up at some future period."

As we also noticed, in our last Number, the public retraction of the celebrated Dr. Rush in reference to the same question, we will here make a permanent record of the circumstances connected with that event. The account is by Professor Caldwell, of Louisville, Kentucky; and in making this extract from a private letter, we trust that we have not exceeded the bounds of propriety.

"From your last letter and last Journal, I have learnt that you have been recently engaged in the same sort of contest, respecting the same topics, and with the same success, which fell to my lot, six or seven times in Philadelphia; and at a period, not a few years before you were born. I allude to your late inquiries after, and your excellent and conclusive analysis of, the Rondout epidemic, or endemic, according as the one name or the other may be most applicable to it.

"I commenced my yellow fever campaign in 1793, when a private pupil under Dr. Rush, and did not discharge my last shot on the subject until about twelve or fifteen years ago, or perhaps less. I assumed and defended, from the beginning, the positions of domestic origin and non-contagiousness; and therefore non importability. On the subject of the contagiousness of the pestilence, my great preceptor was inexorably opposed to me, until about the year 1807, when he gave in his adhesion to the true doctrine, and never afterwards opposed it.

"In relation to this point, nineteen out of twenty, or perhaps a much larger proportion of physicians and others, are deceived. They believe Dr. Rush to have been the author of the doctrine of non-contagiousness. No such thing. He was the author of domestic origin. But, for many years, he was the most flagrant antagonist I ever knew—and did not, I say, surrender the error, until about the year 1807. He surrendered it, moreover, to Dr. Physick and myself, in the course

of a long and earnest conversation, and, on our part, of a very energetic remonstrance.

"When the Doctor pronounced his abandonment of the doctrine of contagion, I begged him to commit it to paper. He did so, in a few lines, which I despatched by the first following mail, to the Editors of the New York Medical Repository, and had it published in the next number of that excellent periodical. This is a veritable account of Dr. Rush's adhesion to, and rejection of, the groundless notion of yellow fever contagiousness."

We have recently looked over some of the older writers of this city upon the subject of yellow fever; as, for instance, Dr. Richard Bayley's account of the epidemic of 1795, and Dr. Edward Miller's Report upon that of 1805, addressed to the Governor of the State. What has struck us as the most prominent feature in these papers, making due allowance for the additional light thrown upon many collateral points in the interim, is the clear manner in which they review the facts which came under their observation, uninfluenced by the prejudice of preconceived opinions. After adducing incontrovertible facts showing the non-contagious nature of yellow fever, Dr. Bayley says:

"In confirmation of this, we have the declaration of the Medical Society, in answer to a letter from Governor Jay, dated 24th of September, 1795:—'The collective opinion of this Society, as the result of attentive observation, is, that the fever is not specifically contagious; and we are confirmed in this opinion from the following powerful considerations, that neither relations, nurses, nor physicians, have, save in one or two doubtful cases, been infected by attending on those who have either recovered or died of the disease.'"

In reference to the epidemic fever of 1795, Dr. B. further remarks:

"It will be proper to remark here, that the epidemic of last year is not a new disease. It is established upon respectable authority, that a disorder, in every respect similar to the one which made its appearance in this city in July last, and in one instance attended with the symptom of black vomiting, occurred in Water-street, in the neighborhood of Dover-street, four or five years ago, and more or less every year since; and was particularly confined to that part of the city; but being unaided by a particular state of the air, and under the influence, perhaps, of exciting causes of a less active nature, it was neither so generally felt, nor so fatal in its operation.

"Agreeably to this opinion is the declaration contained in a letter we before have had occasion to quote; the author of which observes,—'*I am convinced that the disorder has originated from local causes, because it has appeared in this quarter of the city, at the same season, for several years past.*'

"Nor has this disease been confined to New York. It is a fact beyond controversy, that during the warmer parts of the year 1795, a fever, in all essential circumstances resembling the epidemic of this place, appeared in many places in this State and in the State of Connecticut."

The "Report" of Prof. Miller, who was then the Resident Physician of the city, was received with the most marked favor on both sides of the Atlantic. It was reprinted in Great Britain, translated into the French and German, and inserted by M. Dumeril, at full length, in his Report on the Spanish Epidemic of 1804, addressed to the Emperor. As regards the questions of the local origin and non-contagious character of the disease, the affirmative was most triumphantly sustained.

We would also refer to the yellow fever that prevailed in this city in 1741 and 1742, with the view merely to give the following quotation, which illus-

trates the endemic character of disease, a century ago, in Ulster county—the seat of the malignant fever in the past summer. The Wallkill stream here mentioned, empties into the Rondout seven miles above the village of the same name. Of this locality, in 1743, the Hon. Cadwallader Colden, as appears by the "Medical Repository," wrote thus:

"It is well known that the Paltz-river, or Wallkill, in Ulster county in this Province, has been long taken notice of as very prejudicial to the health of those who live near the banks of it. The waters of this river are of a dark color, and are known to come from a large space of ground overflowed with stagnating waters. The inhabitants along this river are well known to be yearly afflicted with intermittent fevers during the summer season; and a constant fog or vapor is observed almost all summer, except in the time while north-west or northerly winds blow, to arise over that river, and to remain there to a certain height and distance, every morning, till the heat of the sun disperses it; and frequently likewise in the evening. I have observed these intermittents to have different degrees of malignity in different years; and that these last two years they have been more malignant than usual."

In our account of the malignant fever at Rondout, in our last Number, we adverted to the philosophy of epidemics, showing that they succeed one another in a series. It has been long known that different forms of epidemics most usually succeed each other consecutively, either in the same year or in different years; and this was designated by Sydenham, an epidemic constitution of the atmosphere. It is, indeed, a rare circumstance, that any one form of epidemic disease rages alone. Hence we find that the malignant remittent fever of Rondout was preceded by the epidemic influenza, which, for even two months before the arrival of the schooner Vanda, which, it was alleged, introduced the fever, was often characterized by the symptoms of great prostration of the vital powers, preternatural slowness of the pulse, yellow suffusion of the skin and eye, and the vomiting of a dark-greenish fluid, with flocculi floating in it. But this law of epidemics has been still further illustrated; for there prevails, at this time, both in Ulster and the adjoining county of Orange, a violent form of *scarlatina* of the congestive variety. There occur here those rare cases, in which the eruption, after having nearly run its course, returns just about the stage of desquamation. The application of blisters or sinapisms, is invariably followed by mortification; and death often takes place within from three to twenty hours from the first attack.

Nor has the *epidemic constitution* of the atmosphere wholly disappeared elsewhere; for there now prevails (December) in the cities of New York and Boston, a re-visitation of epidemic influenza, though in the latter a much less general and severe form than in July last. We thus have an illustration of the now well-known law, that epidemics, in their unknown cycles, appear in a consecutive series.

Lastly, we will notice the recent production of the distinguished Fergusson, in the Edinburgh Medical and Surgical Journal, for July, 1843; and in making the following extracts, our only regret is, that we cannot lay before our readers the whole of this truthful and cogent argument:

"A body of European troops arrives in a West India colony, and soon after the yellow fever breaks out amongst them. The seasoned Creolized white inhabitants feel little or nothing of it, and the colored classes, without exception, the most numerous, by at least ten to one of the inhabitants, with whom all

strangers are ever in necessary close communication, stand by absolutely untouched. How is this? Import small-pox or any truly contagious disease, and they will suffer far beyond the usual sufferings of Europeans. Take them to England, they will be as liable as ourselves to fall under the dominion of typhus fever; to the Levant, under that of the plague; of psora and syphilis, and every other infection, they will have their full shares. Yellow fever alone they cannot take; and how is this to be explained but upon the obvious fact of that disease being a seasoning fever of malignant type, peculiar in a great degree to newly-arrived Europeans; the product of high temperature and unwholesome locality alone, which, when they come in the crowds of a military expedition, will devastate with all the fury of pestilence winged on the false terrors of contagion, to which the imitative tendencies and involuntary sympathies of our frames, have in every epidemic, when once begun, added tenfold force; but let us here examine somewhat more closely.

"One would naturally suppose that, wherever a true infection existed, the hospital where all the sick are congregated would not be a place of safety, yet there, notwithstanding, is it most likely to be found. Apply this touchstone, it will be seen that the medical officers never suffer more from the disease than their fair proportion, according to numbers; and the more immediate white attendants, orderlies, and others, uniformly less, if the ventilation and discipline be good, than the soldiers in barracks who never go near the place; because, while so employed, they are saved from exposure to the sun's heat, to night guards, and drunkenness. This I proved from incontestable hospital returns when I was last in the West Indies, as also, that the supposed contagion was never communicated to the surgical sick, the convalescent and others, although occupying the most contiguous beds in the same hospital.

"Take another proof. A sugar ship loading at anchor, or a transport, will, as hundreds and thousands have done, lose part of her crew by yellow fever. At last she sails full of the *fomites*, or even of the persons of the diseased. Now, here is the assumed contagion fairly impounded without escape, and one would suppose the disease would then do its worst; yet it does not. It uniformly stops—the sick recover every day the ship sails to the northward; she cannot, in a clear, uninterrupted passage, carry the pestilence beyond the tropic, and unless her destination be Gibraltar, Cadiz, or some other port, already under the influence of the disease at the close of the advanced summer season, where the heat has long been equatorial and the atmosphere (a European one) in all probability stagnant, and as well adapted to reproduce it as any part of the West Indies, she will arrive as free from contagion as the first day she left the stocks. But this will not satisfy the quarantine master. He has been placed there to find contagion, and, as has happened more than once at Gibraltar, will be sorely puzzled to account for the aggravated remittent fevers approaching to, or altogether the yellow, occurring before his eyes, when this fortunate chance from the West Indies will at once enable him to cry pestilence, excommunication, importation, fumigation,* and all the mummery practised by that class of officials. Try again:

"In the crowded transports at the beginning of last war, typhus fevers were frequent amongst newly-embarked troops, and they, of course, according to the doctrine of the day, carried the contagion to the West Indies, which afterwards became yellow fever under that exalted temperature. Here at last was importation proved, and *typhus flavus*, *typhus icterodes*, or *icteroideus*, became the true orthodox creed. But alas for the faith: typhus fever will not stand carrying to warmer latitudes. If the ship be even ordinarily clean it will vanish long before you can have entered the tropics, and you may as well attempt to transplant a willow tree or a hazel into a West India colony, as a fever of that class. I do not utterly deny, although I never saw it, the possibility of landing typhus

* On my last return from the West Indies, in the year 1817, I was subjected to personal fumigation on three successive days, at the quarantine station, Isle of Wight, lest I should introduce the yellow fever into England.

fever there from out of a foul, crowded ship, but it will not stay; it will disappear infallibly as soon as ventilation, even in the most ordinary degree, is restored. It is not to be denied that crowded convict and emigrant ships have carried typhus fever through the tropics to the Cape of Good Hope and Australia, but no importation, however foul, could establish it in these countries, or cause it to spread, nor is it, I believe, known in them even now. Seeing then, that neither will our typhus admit of being carried to the West Indies, nor will their yellow demon be induced to visit our shores, that each will preserve its own locality and field of operation, we come again to the question what is it? There is much in a name, and the French have defined it in one word, *la fièvre Européenne*. Well for us had we stuck to this true definition, for it would have saved an infinity of controversy, panic, and delusion. It is in a great degree peculiar to the western world, for we have no such burst of yellow fever in the East, nor, I believe, on the shores of the Southern Pacific, as those which periodically sweep over our western colonies; but it is not unknown in the former, for one of its earliest designations was the *maladie de Siam*, although it never can be called the same destroying scourge that afflicts the western world."

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"No experienced man, unblinded by the prejudices of the schools and authorities, or unbiassed by the expectation of quarantine office, can seriously believe it to be a contagion. It is a terrestrial poison which high atmospheric heat generates among the newly-arrived, and without that heat it cannot exist; but it affects no one from proximity to the diseased, and cannot be conveyed to any low temperature. This was finely exemplified at Port au Prince, St. Domingo, where I spent the earlier months of the year 1796. Our head-quarters were the town and its adjunct, Brizzoton, as pestiferous as any in the world, and there we had constant yellow fever in all its fury. At the distance of a mile or two, on the ascent up the country, stood our first post of Torjean, where the yellow fever appeared to break off into a milder type of remittent. Higher up was the post of Grenier, where concentrated remittent was rare, and milder intermittent with dysentery, the prevalent form of disease; and higher still was Fourmier, where remittent was unknown, intermittent uncommon, but phagedenic ulcers so frequent as to constitute a most formidable type of disease; and higher still were the mountains above L'Arkahye, of greater elevation than any of them, far off, but within sight, low down in what was called the bight of Leogane, where a British detachment had always enjoyed absolute European health, only it might be called better, because the climate was more equable than in the higher latitudes. Here were the separate regions or zones of intertropical health mapped out to our view as distinctly as if it had been done by the draughtsman. Taking Port au Prince for the point of departure, the three first could be traversed in the course of a morning's ride. We could pass from the one to the other, and with a thermometer, might have accurately noted the locale of disease, according to the descending scale, without asking a question amongst the troops who held the posts; and what kind of contagion must that be, which, amongst men in necessary intercommunication, cannot be conveyed from the one to the other; which refuses to mingle with another of lower temperature, although within sight, and so near, topographically speaking, as almost to touch. The men could, and did constantly, exchange duties but not diseases; and it was just as impossible, and more so, to carry a yellow fever up the hill to the post in sight, as it would have been to escape, had they been brought down and located amidst the swamps of Port au Prince. These things were known to every person in the army, whether medical, civilian, or military, and amongst them all there was not to be found a single person who had the smallest belief in contagion, provided always he had been a year in the country, and possessed opportunity of seeing with his own eyes; all, I may say, came out contagionists,*

* The vagaries of these strangers were at times very amusing. A newly-arrived contagionist physician, struck with the great mortality on his sick list, despite of his very best prescriptions, was fortunate enough at once to hit upon the true reason; they had all been feeding—chewing the cud as it were, on their own poison. Accord

myself amongst the number, none remained so. It was impossible that we could, in face of the every-day experience of our lives, and if we had, the very women and drummers of the army would have laughed us to scorn, because they had long discovered there was not the smallest danger to be apprehended from the closest proximity of the sick. The woman knew that when she slept with and attended her dying husband, she had not been infected, and all were sensible that no safer duty could be imposed than that of attending the sick bed."

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"The yellow fever cannot be contagious, because the colored races, whatever may be their exposure to the supposed infection, never take it; nor does approximation and communication with the sick, cause Europeans to be in the smallest degree more liable to fall into the disease than if they had never approached the sick bed.

"It cannot be a contagion because it is restricted to particular localities, temperatures, and elevations. 'Places, not persons,' constitute the rule of its existence. It rarely admits of being carried in the persons of the most highly diseased to other localities, even of the same temperature, if better ventilated; and the villages in the neighborhood of towns where it has been raging, and to which the infected have fled for refuge, have seldom or never become subject to the disease from admission of the strangers. This has been exemplified times out of number, at New Orleans, Vera Cruz, Gibraltar, and Cadiz. Places, not persons, comprehend the whole history—the etiology of the disease. The persons of the sick are always safe to approach—their habitations never. Change the last, the disease is certainly left behind; remove the first, and you at once annihilate the power of infecting others at the changed residence.

"It cannot be a contagion, because the degree of heat that is found to dissipate the best marked contagious fevers, is the life-blood of this. It cannot exist, but at the disinfecting point, and when the plague is dissipated by the heat and rarity of the atmosphere, this begins. So it is with typhus fever. This last goes out when you enter the tropic, the yellow fever commences. The first-mentioned is a communicable infection in the colder latitudes; the latter, the pure endemic of a hot climate that cannot be transported or communicated upon any other ground.

"Again, places not persons. Let the emphatic words be dinned into the ears of the Lords of the Treasury of Trade and Plantations, until they acquire the force of a creed which will save them hereafter from the absurdity of enforcing a quarantine in England against an amount of solar heat of which its climate is insusceptible. Let them further be repeated in the schools of medicine until their professors become ashamed of imbuing the minds of the young with prejudice and false belief, which, should they ever visit warmer climates, may cause them to be eminently mischievous in vexing the commerce, and deeply and injuriously agitating the public mind of whatever community may have received them. Once more, for the last time, places not persons.

"Turn to the celebrated work of Sir William Pym on the Boulam Fever at Gibraltar in 1815, which has, ever since it was published, so fatally influenced all the health authorities of the country, and at page 51 will be found an illustration so clear and conclusive, of the effects of change, even to the shortest distance, whenever yellow fever prevails, that it well deserves being recorded again for the guidance of all who may hereafter be called upon to act under similar circumstances. He tells us that, when the yellow fever appeared in the town of Gibraltar, he, armed with military authority, removed all the inhabitants of the infected locality, sick and well, with their baggage, to the neutral ground

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ing to the common custom of hospitals the sick lay with their heads to the wall, and the matter of contagion which they threw out at every expiration, rebounding from its hard surface, was again drawn in with their breath, so that it was impossible they could recover. The word was forthwith given, feet to the wall, heads to the centre (the middle of the room), and in this extraordinary position they were found on the following morning when the officer of the day and the inspector made their daily visit.

outside the walls, and that the epidemic forthwith ceased. He ascribes the deliverance to purification of the houses, and so forth; the philosopher would ascribe it to removal from malarious site. The conception was erroneous, to say the least of it; the execution so salutary that it ought to be held up for imitation in all times to come.*

"Yet, allowing all the foregoing to be incontrovertible, which it certainly is, still is there much of the unfathomable in regard to yellow fever; its occult sources; its appalling outbreaks, often without preliminary warning; the absence of gradation in the types of fever previous to the grand explosion; its being restricted to the European races, and being confined almost entirely to the Western world, mark it as a disease equally strange and terrible. The idle stories of its being imported are negatived at once by the impossibility of conveying it, as we have just seen, even in the persons of the sick, to the shortest distances; from the inside town of Gibraltar to the walls outside. Wherever it appears, it is generated by the sun's heat from the soil of the place, and it cannot be sent or carried to another, for every spot must have its own, or none. Stagnation of atmosphere accounts for much in regard to it, but not for all; vegetable putrefaction for little or none; nor does malaria for the whole, unless it be some occult malaria of the Atlantic shores; for prolific as the eastern tropic may be, and certainly is, of malaria in all its forms, it rarely produces the epidemic characterized by the leading symptom of black vomit. *Pestis occidentalis* would be a good designation were it not for the superstitious alarm conveyed in the first word; and *fièvre Européenne* is far better. Establish this, and the question would be solved in as far as the West Indies are concerned. Gibraltar and Cadiz and the towns of North America are not so easily disposed of; but tell the importers that, while true contagion, such as that of plague and typhus fever, is never introduced into any of them—at least has not been for more than 150 years—yellow fever appears periodically, and that, instead of looking for the coming of the pestilence from beyond the Atlantic, they should turn their attention to the sun that beams above them, to the ground on which they stand, and the qualities of the soil under their feet.

"It is there the poison lurks and has ever lurked. Fly from it certainly, and enjoin all who have the means to fly the place. To pen up the inhabitants upon the infected ground is to aggravate the disease a thousand fold, and is, in fact, as cruel and absurd as it would be to barricade the doors against the escape of the inmates of a house that had taken fire, on the insane pretence that they would otherwise spread the conflagration. The quarantine authorities will, no doubt, interpose to save the world from the dire contagion, but let them be referred to the annual epidemic at New Orleans for information how often the yellow fever has been conveyed from thence to the upper settlements on the same river, to which the fugitives, sick and well, uniformly fly for refuge, or even to the steamers that carried them; how often at Vera Cruz it has been carried out of the town, even to the first stage on the road into the interior; or how often in Spain it has ever been transported, except to another station under the same circumstances of heat and drought and defective perflation, therefore

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* His words are, "I directed a sufficient number of tents to be pitched outside the gates of the garrison, on the neutral ground, with a proportion of bedding, &c., and in the middle of the night, without any previous information being communicated to the inhabitants, a strong guard, with a sufficient number of carts, proceeded to the infected district, and conveyed the different individuals of the infected families, sick and well, with their baggage, to the encampment, which was established as a lazaret and kept in quarantine. I placed the sick in separate tents, and appointed persons to attend them who had had the disease at a former period. A cordon of troops was established round the infected part of the south district, which was kept in quarantine for fourteen days; proper persons were also appointed to superintend the purification of the houses, furniture, &c., and to report the slightest appearance of disease among any of the inhabitants, who were also paraded and inspected daily by one of the hospital staff."

falling, if not having already fallen, into the same predicament from the same causes. Let them inquire whether seclusion and shutting up has ever saved the terror-struck. In the army of St. Domingo, it was notorious that they were ever the first to be taken ill and the surest to die; and during the yellow fever epidemic of 1816 at Barbadoes, I have recorded remarkable instances of the same, both from my own observation and that of others. In short, whenever the endemic agency could not be avoided, the best place of safety was often to be found in the sick apartment, for there quietude reigned, and fatigue, exposure, and intemperance were most likely to be avoided."

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"Ships have often arrived in the ports and localities where the yellow fever was endemic, with that disease on board, but, unless they came at the season of its epidemic spread, their advent would be as harmless as that of the healthful breeze of the sea. It never yet was imported as an infection anywhere, and while the disease continues as it is, never will be in this world. If it was not there before they came, they could not bring it, and if it was, the imputation would be a needless work of supererogation, as needless as that described in the homely saying of carrying coals to Newcastle."

It may be well to add that Dr. Fergusson here uses the terms, contagion and infection, as altogether synonymous.

Art. X.—*An Experimental and Critical Inquiry into the Nature and Treatment of Wounds of the Intestines; illustrated by engravings.* By Samuel D. Gross, M. D., Professor of Surgery in the Louisville Medical Institute, etc. Louisville: Prentice & Weissenger, 1843. 8vo., pp. 220.

To him who extends the domain of medical knowledge by original investigations, is due the highest meed of the faithful student of nature; and in the volume before us, we see this spirit at work both in its conception and execution. Happily combining an extensive knowledge of pathology, both theoretical and practical, and the industrious spirit which has urged our author on, during two years, to the performance of upward of seventy experiments upon live animals, it is not surprising that much new light has been thrown upon a question heretofore unsettled.

In our surgical literature, a monograph on wounds of the intestines has long been an acknowledged desideratum, inasmuch as the only production of the kind—the work of Mr. Travers—has been out of print for the last thirty years. Besides, as the information supplied by the systematic surgical treatises of the day, as well as by the various periodicals and the transactions of societies, is of the most meager character, Dr. Gross deserves especial credit for his successful efforts in filling up this hiatus in surgical literature. We have here, independent of the results warranted by his own experimental researches, a full summary of the existing state of the science relative to the interesting and important subject of which it treats.

"The experiments which form the basis of the present treatise were commenced in the spring of 1841, and continued, with various intermissions, until a few months ago. The object was, in the first place, to inquire into the process employed by nature in repairing wounds of the intestinal tube; and secondly, and more particularly, to determine, if possible, the value of the more important methods of treatment recommended by surgeons from the time of Ramdohr

down to our own. The experiments, amounting altogether to upwards of seventy in number, were performed exclusively upon the dog, as the most eligible animal that could be procured for the purpose. The wound was generally made in the small bowel, not only because it is the more accessible portion of the alimentary tube, but because it is more liable, when thus injured, to become the seat of fæcal effusion, and also, perhaps, of high inflammation."

In the eye of the pathological and operative surgeon, what topic of inquiry can be more interesting, than the nature and treatment of wounds of the intestines? Considering the different methods adopted by the surgeons of the several countries of Europe, for obtaining adhesion between the edges of a cut intestine, the value of these experimental researches is particularly obvious in the materials furnished for a correct prognosis, and for an estimate of the most successful mode of treatment. As regards the latter, when we behold the errors of such men as Scarpa and John Bell, clearly exposed on the ground of experimental observation, our estimate of the labors of Dr. Gross must be still further enhanced.

Our author divides his monograph into four chapters. 1. *Nature of Wounds of the Intestines;* 2. *Treatment of Wounds of the Intestines;* 3. *Partial and complete Division of the Intestines;* and 4. *Artificial Anus.* Did space allow, we would gladly lay before our readers a detailed view of these chapters; but we trust that this notice of its general features will induce them to obtain the work itself. The various modes of uniting by suture wounds of the intestines are fully described by Dr. Gross, who renders them still more intelligible by calling to his aid, wood-cuts. There is also a colored lithographic plate, which exhibits the appearances of the internal and external surfaces of the intestines after the various experiments performed upon the poor dogs sacrificed in the cause of science and humanity.

This little volume speaks trumpet-tongued in favor of the industry and zeal of its intelligent author, fostering the hope that so successful a prosecutor of pathology will continue to gain laurels by the rigid investigation of other mooted questions of equal importance in the rich field of surgical science.

ART. XI.—*A Practical Treatise on the Diseases of Children.* By JAMES STEWART, M.D., A.M., etc. New York: J. & H. G. Langley, 1844. 8vo. pp. 544.

THE simple fact that a new edition of this work has been demanded, is certainly the most satisfactory evidence of its favorable reception; and in complying with this demand, Dr. Stewart has not failed to keep pace with the progress of this department of medicine since the period of his prior publication, as regards every improvement both in theory and practice.

In the rapid advancement, in recent years, of the whole circle of the sciences, by which the new application of principles is constantly developed, many prominent theories with their practical deductions have sprung into existence; and to present a digest of these in a condensation of

existing knowledge, is what the professional public require from time to time. In this effort, so far as the diseases of Children are concerned, our author has been very successful.

We like much the arrangement of the subject adopted in this work. The leading divisions are three, namely, the *Vital Functions*, the *Natural Functions*, and the *Animal Functions*. Under the first head, come the *Respiratory and Circulating Systems;* under the second, the *Digestive and Excernent Systems;* and under the third, the *Nervous and Motor Systems*.

Let not the reader, however, suppose that this work is a mere compilation. Far from it. It consists, it is true, mainly of the recorded experience of the past, without which no medical work, we are sure, would be deserving of the time requisite for its perusal. As an evidence of the author's own sources of information, and of the good sense displayed in the selection and arrangement of his facts, whether his own or obtained from the recorded experience of others, we would refer particularly to the sections on variola, vaccinia, and vaccination.

We coincide in the opinion of the author, that the experience of the last forty-five years, "accumulated from every part of the world, has fully proved the triumph of vaccination over the dreaded contagion of variola." This question is exhibited by Doctor Stewart in its true light, showing that the proper development of the protecting disease in the system may be prevented by several causes, which are sufficient to explain the prevalence of varioloid affections. But our limited space compels us to pass over the details of these causes which may render the protective powers of vaccination, in particular individuals, nugatory. The difficulties connected with the question of the varioloid, arise from the impracticability of determining whether an individual has been properly vaccinated; and if properly done, whether the progress of the constitutional affection may not have been interfered with, by the influence at the time of some morbid action in the system; or thirdly, whether the second attack may not be due to the peculiar idiosyncracy of the patient.

Our author exhibits, by a comparison of the mortality caused by small-pox, prior to the introduction of vaccination and subsequently to that event, the most convincing proofs of the triumphs of the cow-pox, as a prophylactic agent. But not content with this mass of evidence, which has accumulated to an extent that finds no parallel on any other question of scientific investigation, he has devoted much time in making a personal examination of the records of certain institutions, and especially of orphan asylums.

"The full benefits of vaccination are not to be found by seeking for them indiscriminately among the inhabitants of large cities, who are constantly exposed to the numerous causes which produce failure in the progress of the vaccine vesicle, but among those who enjoy all the benefits of professional skill, not only in the selection of proper virus, but also in ascertaining the proper development of the constitutional symptoms. We might, therefore, expect to find it in its most perfect state where these requisites are enforced, as in public institutions, and wherever the necessary measures are fully adopted. The following are a few of the results obtained from these sources.

"In the year 1815, the small-pox appeared on board the U. S. Frigate Guerrière. An inquiry was instituted by the surgeon, to ascertain whether those attacked had ever been vaccinated, when it was ascertained that none of them

had ever had it. Not one of the crew who had been vaccinated took the disease in any form.*

"In the different institutions for the reception of children, the greatest care is bestowed on the details of vaccination, and it is under such circumstances that we are to look for the full results of this important preventive measure, up to the age of fourteen years, the average period at which the children are discharged.

"In the Orphan Asylum of Charleston, S. C., which, in the year 1829, contained one hundred and fifty children, not a single case of small-pox or varioloid occurred during the prevalence of that disease, although no additional restriction was imposed upon their intercourse with the citizens.†

"The aggregate number of children received into the different orphan asylums of Philadelphia, since their establishment to the beginning of the year 1841, is 1,009, and among the whole, there has been but one death from small-pox; this occurred some years since, out of sixty-five cases of the disease. The children were carefully examined, both with reference to the virulence of the affection, and to their condition at the time of the attack. Ten were without the usual cicatrix left by the vaccine vesicle, and the child that died was one of the number destitute of this mark.

"In the city of New York, the total number received in all the orphan asylums, is 2,384; and although the small-pox appeared in two or three of them, yet it was in a greatly modified form, and no deaths have occurred from this cause. A similar result is obtained, also, from the House of Refuge, which exhibits an aggregate number of 2,657 children received during sixteen years. The whole number, therefore, that have been inmates of these various institutions in the city of New York, is 5,041, and not a single death from small-pox has at any time occurred.

"Baltimore, also, presents the like return, out of 3,500 children that have been for several years in the alms-house and different orphan asylums of that city.

"The entire number of children, therefore, that have been under the care of the above-mentioned institutions, since their establishment, is 9,550, and but one death has occurred from small-pox in the course of thirty years; and this one it is fair to presume had not been vaccinated, as no mark of this operation was found after the most careful examination. The aggregate number that have been received in the above-named institutions amounts at this time, November, 1843, to upward of 11,000, without any variation in the results of vaccination already stated.

"The results of vaccination thus ascertained, can not be followed after the children have left these institutions, but up to that time the benefits may be learned by comparing with them the effects of small-pox before the introduction of vaccination; its ravages being principally among the young. At this day, also, the small-pox is more prevalent and fatal among children; as appears from the city inspector's report of deaths in the city of New York, for the last few years. Ninety-one deaths are reported to have occurred from this cause, in the year 1838; sixty-nine of these were children under ten years; and in 1839, sixty-eight deaths were reported, forty-five of which were among children of the same ages; and of 231 deaths from the same cause in the year 1840, 131 were also children not yet arrived at the age of ten years. To whatever cause this number of deaths is to be attributed, whether to an imperfect vaccination, or its neglect altogether, the comparison shows the triumphant results of the prophylactic measure now universally adopted.

"The value of the discovery of vaccination, as was before remarked, does not consist in its entirely preventing an attack of small-pox, but in disarming it of its terrors—in reducing the mortality, which it has been seen makes it one

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* Report of the Med. Soc. of N. Y., 1817.

† Report of the Committee of the Board of Health of Charleston, etc.; Amer. Journ. Med. Sciences, November, 1831.

of the most fatal scourges of mankind, to a very small amount; a result not obtained even by the small-pox itself, many more deaths having, as has been sometimes observed, occurred among individuals after having passed through that disease, than among those who had been protected by vaccination. The recorded statistics of Drs. Mitchell and Bell, of the results of their experience during the epidemic of Philadelphia, in 1823 and 1824, also prove this fact, which has been noticed by others. Of 248 cases of small-pox and varioloid, 155 were unprotected, of whom eighty-five died; sixty-four *vaccinated*, of whom one died; nine *inoculated*, of whom three died; seven previous small-pox, of whom three died; thirteen unknown; no deaths.*

"When these results of the two methods are considered, how striking are the advantages of vaccination, not only in saving human life, but also in its direct tendency to exterminate a loathsome malady altogether; and when the many causes which are known to exist that influence the proper development of the vaccine disease, and the carelessness which must prevail in the thousands of instances of vaccination, are taken into account, far from having our confidence lessened, when the results do not accord with our most sanguine wishes, these circumstances should on the contrary strengthen our faith in the salutary influence of a remedy, which still must be regarded as one of the greatest blessings to man."

And at the end of the volume, we find the following additional evidence upon this point:

"At the request of the author, Dr. Morrell, physician at the Long Island Farms, a department of the New York Alms-House where children are received, kindly furnished him with the statistics of the cases of varioloid which occurred at that establishment from the first of November, 1834, to the first of May, 1843, comprising a period of eight years and a half. From this, it appears that of 5,856 children who were received during that period, there were but eight deaths from varioloid, among those in whom it was ascertained that vaccination had been practised. This result, added to that already stated at page 467, exhibits an aggregate of nearly 16,000 children vaccinated, and but eight deaths among them from the effects of small-pox contagion."

In view of the general character of the volume before us, comprising, as it does, a condensation of the existing knowledge upon the subjects of which it treats, we venture to predict, guided also by the history of its past career, that it will become a work of still more extended reference as a standard production.

ART. XII.—*On the Effects of Secluded and Gloomy Imprisonment on Individuals of the African Variety of Mankind, in the production of Disease.* By BENJAMIN H. COATES, M. D. Read at the Centennial Anniversary of the American Philosophical Society, May 29th, 1843. 8vo. pp. 14.

OUR author having been for many years an official visiter of the Eastern Penitentiary of Pennsylvania, his attention became attracted to the great inequality in the operation of separate and gloomy confinement on individuals of the white and the colored varieties of the human family. We

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* N. A. Medical and Surgical Journal, vol. ii.

need not say that the existence of a disproportionate mortality, under these circumstances, is a subject of interest and importance both in a moral and scientific point of view. The statistics of this Penitentiary exhibit the result, "that the average mortality of white convicts in the prison was less than that of the white inhabitants of the City and Liberties of Philadelphia; while that of the colored prisoners greatly exceeded the mortality of either white or colored among our general population."

The following results, during the whole duration of the prison, from 1830 to 1842 inclusive, being the average populations of the whole thirteen years, compared with the total number of deaths, are as follows:

Total white averages, 13 years, with the fractions given,	2073·60
Total white deaths, 13 years,	42·
Average rate per cent. of white deaths in 13 years,	2·03
Total colored averages, 13 years,	1323·40
Total colored deaths, 13 years,	93·
Average rate per cent. of colored deaths, 13 years,	7·03

Dr. Coates now compares these results with the comparative mortality of the white and colored races in Philadelphia and its suburbs; but it is to be regretted that the following table, compiled from the researches of Dr. Emerson, does not embrace the same years as the table given above:

	Whites.	Colored.	Whites, per cent.	Colored, per cent.
1821,	1 in 49·1	1 in 16·9	2·31	5·92
1822,	" 41·9	" 21·5	2·39	4·65
1823,	" 33·8	" 17·5	2·96	5·71
1824,	" 35·1	" 17·5	2·85	5·71
1825,	" 42·4	" 27·0	2·36	3·70
1826,	" 40·3	" 26·1	2·48	3·83
1827,	" 47·4	" 18·9	2·11	5·29
1828,	" 43·6	" 20·8	2·29	4·81
1829,	" 44·0	" 23·7	2·27	4·22
1830,	" 45·4	" 27·2	2·20	3·68

"To draw an average of the whole which should be absolutely correct, it would be necessary to have the whole numbers These are not given by Dr. Emerson. The averages of the above ratios are, for the whole ten years, whites, 2·422; blacks, 4·752; and these are probably near the truth.

"Now these numbers are to each other in the proportion of 1 to 1·96. That is, out of an equal number of each complexion residing in our city and suburbs, to every hundred deaths of white persons there die one hundred and ninety-six colored persons. The deaths above given as occurring in the prison (whites 2·03, colored, 7·03), are to each other as 1 to 3·46: or, out of an equal number of both complexions residing in the Penitentiary, to every hundred deaths of white persons there would die three hundred and forty-six colored persons!"

This shows a like disproportion of mortality both in and out of the Penitentiary—a result that may be justly ascribed, in part to a difference of mode of life, but more especially to the influence of a climate which is not natural to the African variety of the human race. It may be well to present these relative ratios more in contrast, by approximating them:

	Per cent.
White mortality in Penitentiary, 13 years,	2·03
White mortality in city and suburbs, 10 years,	2·422
Colored mortality in city and suburbs, 10 years,	4·752
Colored mortality in Penitentiary, 13 years,	7·03

"If Dr. Emerson's average of white deaths in the city and suburbs be assumed as unity, these numbers will then be represented as follows:—

Whites in Penitentiary,	·838
Whites in city and suburbs,	1·
Colored in city and suburbs,	1·962
Colored in Penitentiary,	2·903

But here we must bear in mind that this table affords no relative comparison of mortality in the prison and in the city; for as convicts are generally in the prime of life, the prison, unlike the general population of a city, is exempt from the high mortality incident to old age and to infancy. The fact, however, that the white convicts exhibit a lower ratio of mortality than the same race among the population of Philadelphia and suburbs, while the colored convicts, similarly compared, present a much higher, establishes satisfactorily a wide difference in these two varieties of mankind, as regards the effects of imprisonment.

Granting these results to be clearly established, Dr. C. thus explains and confirms them physiologically and pathologically:

"The negro, or even the mulatto, is a very different person, in his physical and psychical conformation, from that one who may be presumed to have been held in view in our legislation, the white Anglo-Saxon, Celt, or German. His ancestry, and the prototype of his race, are calculated for the torrid zone; and even the mixed progeny suffer severely and mortally by our cold. Cheerful, merry, lounging, and careless, the Ethiopian American deeply enjoys the sun and light; delights in the open air; and is, as a general rule, constitutionally free from that deep, thoughtful anxiety for the future, so conspicuous in his paler neighbor. The face of heaven seems to him necessary to his existence; and though long confinement is, in his case, less productive of gloomy remorse, it is far more depressing to his vitality.

"The morbid effects of this are unhappily visible in the production of scrofula and pulmonary consumption; more than eighty-eight per cent. of the deaths being from chronic affections of the lungs, and from the first-named disorder. The moral consequences are, in an equivalent degree, depressing to the mind. It is not by remorse and anguish that he is affected, so much as by intellectual and moral weakness and decay; and gloomy confinement becomes thus to him, mentally as well as physically, a nearer approach to the punishment of death. The effect of separate imprisonment has not been, as has been erroneously charged against it, to produce insanity, although a humane and strict analysis has shown many to have been affected both with insanity and with imbecility, at the times when they committed the offences for which they were sentenced. The effect upon the unfortunate colored prisoners, though scarcely perceptible upon the whites, has been to produce not mania, but weakness of mind; dementia, instead of deranged excitement.

"Throughout this inquiry, I have generally preferred reasoning from the deaths, partly from the force of the consideration, but principally, as regards the scientific question, for the sake of the greater mathematical precision of the results. We have, in the reports for 1837, 1838, 1839, and 1842, a detail of the mortal diseases and immediate causes of these, in forty-three cases of colored persons. These are as follows:—

Consumption and chronic inflammation of the lungs; 1837, 6 cases; 1838, 12; 1839, 1; 1842, 1,	20
Scrofula of the chest, 1838, 1,	1
Chronic pleurisy, 1838, 2 cases; also affected with chronic inflammation of the stomach, or with that of the bladder, and with paralysis: 1839, 5; of which 1 was cut off by brain fever; 1842, 1,	8

Scrofula of other parts than the chest, 1837, 2 cases: 1838, 4, including affections of peritoneum, bowels, and knee joint; 1839, 2, including peritoneum and hip joint; and 1842, 2,	10
Typhus fever, 1837,	1
Remittent fever, 1837,	1
Asthenia, 1842,	1
Tetanus, from a burn, 1842,	1
Total,	43

"Vicious habits are enumerated as causes in fourteen of the cases; and in three of them, they are the only cause assigned. Four are ascribed to previous syphilis; and in one, no other cause is recorded.

"Of nineteen deaths of white prisoners, during the same years, the diseases were as follows:—

Consumption, 1837, 5 cases; 1838, 3 cases,	8
Pulmonary and hip diseases, 1842,	1
Brain fever, reported as owing to scrofula and disorganized lungs, 1837,	1
Syphilitic chronic pleurisy, 1839,	1
Scrofula,	0
Syphilis, 1837,	1
Chronic bowel complaints, 1838, 1; 1842, 1,	2
Small-pox, 1838,	2
Asthenic brain fever, 1839,	1
Stone, 1838,	1
Diseased arteries and enlarged heart, 1842,	1
Total,	19

"Vicious habits are assigned as a cause in four cases; and in two are the only cause named. Vice before admission is represented as a cause in four cases; and in two of them is the only one named.

"Of forty-three deaths of colored convicts, twenty-nine are ascribed to chronic diseases of the lungs, and to affections of an adjacent structure, in which these are extremely liable to produce such diseased changes; and ten to scrofula of other parts than the lungs; leaving only four for all other affections; and these four were produced by typhus fever, remittent fever, asthenia, and tetanus. Of nineteen deaths among white persons, on the other hand, the causes are found in chronic diseases of the lungs and their appendages, for eleven cases; scrofula, *none;* chronic bowel complaint, two; small-pox, two; and four others are severally attributed to syphilis, asthenic brain fever, stone, and diseased arteries, with enlarged heart. Reduced to per centage, these proportions would read as follows:—

Colored.

Diseases of chest, exclusive of heart and arteries,	65·12
Scrofula,	23·25
All other diseases,	11·63
Total,	100·

White.

Diseases of chest,	57·89
Scrofula,	0·
All other diseases, including bowel complaints, 10·53, and small-pox, 10·53,	42·11
Total,	100·

This inquiry recalls to mind the question discussed in our penultimate Number, relative to the vital statistics of whites and negroes. Now, as the mortality of the colored population in Philadelphia is twice as high as that of the white, this result cannot be well reconciled with the idea that of the former, compared with one of the latter, thirty-one attain the age of 100 years and upward. We are now, however, engaged in an inquiry which will, we trust, set this question of longevity at rest.

ART XIII.—*A Practical Treatise on the Diseases of the Testis, and of the Spermatic Cord and Scrotum, with Illustrations.* By T. B. CURLING, Lecturer on Surgery and Assistant Surgeon to the London Hospital, Surgeon to the Jews' Hospital, etc. Edited by P. B. GODDARD, M.D., etc. Philadelphia: Carey & Hart. 1843. 8vo. pp. 568.

ALTHOUGH Sir Astley Cooper's work on the testis has supplied a desideratum long felt in the profession, yet, in consequence of its high price, it has been chiefly confined to the wealthy few among the great body of the profession. As the monograph before us, however, is exempt from this objection, and is within the reach of all, its republication here must necessarily meet the approbation of the American medical profession.

The author divides his volume into the four following parts:

I.—Anatomy of the Scrotum and Testis,
II.—Diseases of the Testis,
III.—Diseases of the Spermatic Cord,
IV.—Diseases of the Scrotum.

At first view, it may be supposed that the "Anatomy of the Scrotum and Testis," in a work treating of this subject, is supererogatory; but upon this point, our author remarks:

"I was unwilling to overload a work which has somewhat exceeded the limits desired, with elementary matter to be found in most anatomical treatises; but as my researches on the structure of the testis have led me to describe certain parts rather differently from other anatomists, and have enabled me to throw some light on the interesting subject of the descent of the testis, I have prefixed a concise, but it is hoped sufficiently minute account, of the anatomy of the parts in the adult and fœtal states, which, comprising, as it does, the most recent information on this subject, will probably be acceptable to my younger readers."

Part II. treats of Congenital Imperfections and Malformations, Atrophy of the Testis, Injuries of the Testis, Hydrocele, Hæmatocele, Orchitis, Tubercular Disease of the Testis, Carcinoma of the Testis, Cystic Disease of the Testis, Fibrous Transformation of the Testis, Ossific Deposits in the Testis, Loose Bodies in the Tunica Vaginalis, Spermatocele, Fœtal Remains in the Testis, Entozoa in the Testis, Nervous Affections of the Testis, Sympathetic and Functional Disorders of the Testis, and Castration.

Among these excellently-treated subjects, we scarcely know which to select by way of specimen. Under the head of "Imperfect Descent of the Testis," we find the following explanation of the phenomenon:

"The causes of a failure in the descent of the testis have not been much investigated; and as considerable doubt has long prevailed respecting the mode and agency by which this change is effected, no satisfactory explanation could be expected of the circumstances interrupting or preventing it. When we reflect on the nature of that process, as my researches have led me to describe it, it is clear that there must not only be a perfect adaptation of parts, a due relation between the body drawn down, and the structures which it traverses, but also corresponding power in the agent by which it is accomplished. There are few muscles in the human body whose development in different individuals varies in a greater degree than that of the cremaster. And if such be the case after birth, it is not unreasonable to presume that similar differences exist in the fœtus before the descent of the gland, and that a failure in that process may be the result of deficient power in the musculus testis to accomplish the passage. May we not also conclude that this muscle is sometimes paralyzed, and that the faulty descent is owing to a want of a due supply of the nervous energy which we know is often denied to other muscles during fœtal existence, and is the cause of deformities in the feet and other parts with which infants are often ushered into the world? I think, indeed, we may fairly enumerate paralysis and defective development of the cremaster amongst the causes of the imperfect descent of the testis.

"Peritonitis occasionally attacks the fœtus in utero,* and produces adhesions between the various abdominal viscera. It is well known, that in congenital hernia the testis is frequently united to a portion of intestine or omentum, and that the formation of these adhesions, previous to the testicular descent, is sometimes the cause of the displacement, the viscera being drawn down together with the gland into the scrotum. Many facts seem to show that similar adhesions are, on the other hand, an occasional cause of the temporary and permanent retention of the testicle, the cremaster being insufficient to overcome this obstacle to its descent. In the body of an old man, M. J. Cloquet found the left side of the scrotum empty, and the testicle situated at the distance of an inch from the superior opening of the inguinal canal: the head of the epididymis was connected to the sigmoid flexure of the colon by a strong white fibrous band.† Wrisberg states, that on examining an infant which had only the right testicle in the scrotum, and died a few days after birth, he found the opposite gland close to the ring and connected to the omentum by means of three slender filaments.‡ Dr. Simpson, in the dissection of an anencephalic fœtus, found marks of extensive peritonitis, and the right testicle imbedded in a quantity of coagulable lymph, which strongly attached it to the peritoneal surface of the iliac fossa.§ Jobert remarks, that he once found in the fœtus, the cœcum adherent to the testicle, which was on the point of passing the ring.‖ In the examination of a man aged sixty, who died in the London Hospital, of phthisis, I found the right testis just external to the abdominal ring; it was small in size, and closely adherent to a portion of omentum. A young man, aged nineteen, has been under my care the last twelve months, on account of an imperfect descent of the testis on the left side. The gland plays backwards and forwards through the external abdominal ring. By pressure above, it can be forced down sufficiently to admit of being examined. This testis is much smaller than the right, which is in the scrotum, and I can distinctly make out a portion of intestine closely adherent, which accompanies it in all its movements.

Mr. Hunter remarks, that the part where the testis meets with the greatest difficulty in its descent, is in the division of the tendon of the external oblique muscle, called the ring.¶ It is probable that the smallness of this opening is

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* Vide contributions to Intra-uterine Pathology by Dr. Simpson, Edinb. Med. and Surg. Journal, Nos. cxxxvii. and cxl.

† Recherches sur les Causes et l'Anatomie des Hernies Abdominales, p. 24.

‡ Lib. cit., p. 229. § Edin. Med. and Surg. Journal, No. cxc., p. 27.

‖ Traité des Maladies Chirurgicales du Canal Intestinal, t. ii., p. 332.

¶ Lib. cit., p. 16.

sometimes, also, a cause of the detention of the testis, especially in those cases in which the organ is retained within the inguinal canal. Mr. Wilson, an accurate anatomist, was of this opinion,* which is supported by the fact, that the testis is oftener found in the groin than in the cavity of the abdomen. M. Delasiauve mentions a case, in which he states the organ was retained by the border of the outer column of the ring.†

"Mr. Hunter was inclined to suspect that the fault originates in the testicles themselves. It is difficult to understand how this can be, for, as the gland is passive in this process, it can offer no obstacle, unless it grows too large to pass the opening in the abdominal parietes; whereas, it is admitted that the gland, when retained, is usually below the natural size. Nor does it appear that the interruption is owing to any want of proper length in the vas deferens, for in a case of imperfect descent in a boy, whose body I examined, I particularly noticed that this duct was so long as to be doubled on itself and tortuous, a circumstance which has been remarked in other cases by Mr. Mayo,‡ Rosenmerkel,§ and others. It may be concluded then, that the causes of a failure in the descent of the testis are various; that this imperfection may result from want of power or paralysis of the cremaster muscle, from adhesions retaining the gland within the abdomen, and from a contracted state of the opening of the external abdominal ring."

As regards the belief that the long-continued use of iodine produces an absorption of the testis, our author's experience warrants the negative:

"It is a common belief that wasting of the testis is liable to be induced by the long-continued use of iodine. I have not met with any instance of it, and I know of no case in which the evidence is such as to render it at all clear that the decay of the gland was really occasioned by this remedy.‖ Iodine has been employed much more largely and generally since the time that this opinion was promulgated, so that if it exerts the influence on the testis which has been attributed to it, we might reasonably expect that the fact would have been satisfactorily ascertained by recent experience. Such, however, is not the case; and I feel convinced that the effect of iodine in producing wasting of the testis, if it ever occurs, happens so rarely, that the liability to it cannot be regarded as any objection to the free and long-continued use of this valuable remedy."

In reference to the treatment of Orchitis, many very practical remarks are made:

"The cure of acute orchitis has been much facilitated, of late years, by the application of a mode of treatment which has been found of great service in relieving certain forms of acute inflammation in other parts of the body, viz., *compression*. The object of compression is to afford support to the weakened vessels; and in acute inflammation of the integuments, when properly applied for this purpose, and not so firmly as to produce pressure and arrest the circulation, it often proves a very valuable method of treatment. Dr. Fricke, of Hamburgh, I believe, first suggested the practice of treating both acute and chronic orchitis by compression, applied to the testis by means of adhesive plaster. In an early report of this practice, he states, that out of fifty-one cases of acute orchitis, eighteen were treated by the ordinary means, and thirty-

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* Lectures on the Urinary and Genital Organs, p. 405.

† Revue Médicale, Mars, 1840, p. 363. ‡ Human Physiology, 3d edit., p. 411.

§ Ueber die Radicalcur des in der Weiche liegenden Testikels.

‖ I have heard of a case of a medical gentleman who attributed loss of virile power and a diminution in the size of his testes to the continued use of iodine, but whether justly so, I have no means of inquiring.

three by compression. In the latter cases, the average duration of the disease was nine days, whilst in the former it was thirteen. In cases treated more recently, after improvements had been made in the mode of applying the compression, the result was still more favorable.* This practice has since been adopted in Paris by Cullerier and Ricord; and in this country by Mr. Langston Parker, Mr. Acton, my colleague, Mr. Hamilton, myself, and others, and, I am informed, by the army surgeons generally. Some care is required in making the application, which I perform as follows. The patient being placed in the recumbent position, with the testis raised, is to remain there three or four minutes, in order to allow the vessels of the gland to become as empty as possible. The parts are to be shaved; and some *emplastrum plumbi* must be cut into strips, about three quarters of an inch in width, and eight or nine inches in length. The opposite testis and side of the scrotum being drawn away from the diseased one, so as to render the integuments of the latter quite tense, the first strap is to be placed circularly round the cord, just above the testis, as tightly as the patient can bear it. The second strap is to be placed in an opposite direction, from behind forwards, at the side of the testis near the septum. The third strap is to be applied below the first, so as partly to overlap it; and the fourth in like manner, internal to the second; and so on in succession, until the straps meet, and the whole of the testis is covered, and evenly compressed. A few additional straps may afterwards be applied where most needed to afford support, and keep the others in place; the parts are afterwards to be supported in a suspensory bandage. The strapping generally requires to be re-applied in the course of twenty-four hours. When the patient rises after its application, he feels completely relieved from the aching pain and sense of weight; and patients who have remained in bed in consequence of the pain, have immediately been able to get up and walk about. Some surgeons recommend the application of compression at the onset of the inflammatory attack; and, if the inflammation be moderate, this may be done with advantage, so that, when concealment is desirable, the patient may be able to continue his usual avocations. But, in decidedly acute orchitis, it is better to commence with an active purge, the tartar emetic, and, if necessary, depletion, and to enjoin rest; and then, after twenty-four or forty-eight hours, to resort to compression: for, in active inflammation of the testis, compression is not always sufficient without other antiphlogistic means, and, if solely depended on, may disappoint expectations. But after the more acute symptoms are relieved, compression greatly facilitates the cure, affording relief from pain, causing the rapid subsidence of swelling, and removal of all effused matter. In cases of a chronic character, or to remove the thickening and induration of the epididymis and cord so commonly observed after acute consecutive orchitis, the testis may be strapped with the *emplastrum ammoniaci cum hydrargyro*; or iodine or mercurial ointment may be first applied to the scrotum, and then the strapping, so as to combine the advantages of these applications and of compression. The removal of the chronic enlargement and induration may be further promoted by the exhibition of small doses of mercury. In these cases, I sometimes keep up counter-irritation by painting the scrotum on the side of the affected testis with the following solution—*Iodin.* ʒj. *Potass. Iodid.* ʒss. *Sp. Vin. Rect.* ℥j.; repeating the application every third or fourth day, until the gland is restored to its healthy state. Blisters are sometimes used: but they are too irritating to be applied to the scrotum, and are even liable to cause mortification."

Part III. treats of Varicocele, Adipose Tumors of the Cord, and Spasm of the Cremaster Muscle.

In Part IV., the subjects treated of are the following: Injuries of the Scrotum, Prurigo Scroti, Varicose Veins of the Scrotum, Pneumatocele,

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* Zeitschrift für die Gesammte Medicin, as quoted in the Gazette Médicale de Paris, Année 1836, p. 182.

Œdema Scroti, Diffuse Inflammation of the Scrotum, Mortification of the Scrotum, Elephantiasis Scroti, Hypertrophy of the Scrotum, Cancer Scroti, Melanosis of the Scrotum, Adipose Tumors of the Scrotum, and Fibrous Tumors of the Scrotum.

Under the last subdivision, we find the following case related:

"Dr. Mott, of the United States, excised an enormous diseased mass from the scrotum of a man about seventy-three years of age. The scrotum was twelve to fifteen times its ordinary bulk, and was filled with tumors of a stony hardness, from the size of nutmegs to that of a large pea. The tumors had all a very white appearance; and the integuments over two or three of the largest, having been ulcerated for upwards of a year, poured forth a fetid discharge. A white substance, resembling mortar, was discharged from these openings. The disease was upwards of twenty years' duration, and had been gradually increasing, the tumors multiplying as the scrotum augmented in size. The whole of the disease was removed, and the patient recovered from the operation, and at the end of three years afterwards he was enjoying excellent health.* I suspect this disease was originally a fibrous tumor, similar to the small one which I have described as having been removed from the scrotum by operation. The calcareous matter and other changes resemble those occasionally observed in large fibrous tumors of the uterus."

To Mr. Curling is due the credit of having collected from the best sources, the most useful information upon the various diseases of which he treats; and this work, which is the fruit of the present disposition among medical men to cultivate a particular department of medical science, constituting a *speciality*, must be allowed to be one of the best and most systematic treatises on this subject extant.

The American editor, as well as the respectable publishers, are both equally entitled to much praise, for placing this valuable work within reach of every class of the profession.

Art. XIV.—*Practical Manual of the Diseases of the Heart and Great Vessels.* By F. A. Aran, Interne of the First Class of the Hôtel-Dieu, etc. Translated from the French, by Wm. A. Harris, M. D., Philadelphia: Ed. Barrington & Geo. D. Haswell. 1843. 8vo. pp. 164.

The best evidence in favor of the value of this work is, the fact of its having been re-published in "The Select Medical Library," edited by Dr. John Bell, whose judgment on such a point is one that has but very few equals. We agree fully with the author in the following views:

"Notwithstanding the publication of many valuable treatises on the diseases of the heart, their study is generally neglected.

"The unfounded scepticism which still exists in the minds of many physicians, on the possibility of recognizing and curing these diseases, and, especially, the

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* Philadelphia Journal, as quoted in the London Medical and Physical Journal, vol. lviii., p. 516.

differences of opinion among authors respecting them, have, in no slight degree, contributed to cast disfavor on this useful branch of medicine. Struck with these differences when we began to devote ourselves to the study of cardiac diseases, it seemed to us, that the best means of acquiring a just idea of the whole subject would be, to combine all the materials furnished by authors, and to test by clinical observation the truth of their statements.

"This is the undertaking, these are the investigations in which we engaged, for our own satisfaction in the first place, and which we now offer to the public. The treatises of Senac, of Corvisart, of Laennec, of Bertin, of M. Bouillaud, of M. Gendrin, and especially the excellent work of an observer whom death has prematurely removed from the field of science (Dr. Hope), have been our principal guides. We have not adopted servilely the opinions of any one. We make no pretensions to novelty; but we have compared, and, as well as we have been able, have verified, for ourselves, the observations of others."

As regards the youthful translator, we behold in this effort the germ of future respectability in his profession.

ART. XV.—*Principles of Human Physiology, with their chief applications to Pathology, Hygiène, and Forensic Medicine, especially designed for the use of Students. With over* 100 *illustrations.* By WILLIAM B. CARPENTER, M. D., Lecturer, etc. First American edition, with additions by the author, and notes and additions by MEREDITH CLYMER, M. D., etc., 8vo. pp. 618. Philadelphia: Lea & Blanchard, 1843.

DR. CARPENTER comes before us with no ordinary claims, seeing that he has made this subject a matter of special study, having, in truth, taken pains to test by actual experiment many of the more valuable additions made in recent years to physiological science. Treatises on physiology have of late been rapidly multiplied, which is an evidence that increased attention is being paid to this department of medical science; and this rapid progress of the science requires, in view of the many new facts established day after day, a careful collation of them, at short intervals, with the deductions that they may warrant. Besides, as a consequence of this intimate acquaintance with the entire subject, our author, unlike certain other physiological works, consisting of scarcely more than mere extracts, which are not always harmoniously dove-tailed, makes his materials his own, elaborating them into a homogeneous mass.

We agree with the American editor in the following remarks:

"The peculiar character of Dr. Carpenter's 'Human Physiology,' particularly adapts it to the wants of the medical student. The close connection between Physiology and the practical branches of medicine, has been throughout the work carefully borne in mind, and their intimate relations pointed out. The present Treatise will be found to be a concise, yet comprehensive exposition of the actual condition of physiological science, conceived and executed in a clear and philosophical spirit. With much that is profound and original, the Author has presented all the received or probable facts of the Science of Life, in a well digested and lucid manner, deducing from them legitimate inferences, and carefully abstaining from the discussion of controverted questions, and never hazarding any startling hypothesis."

Did our space allow, it would be a pleasant duty to enter into a minute analysis of the present volume; but in view of the books now on our table, each awaiting an introduction to the reader, a few words must suffice.

In the arrangement of his subjects, our author presents perhaps nothing new, save an excellent preliminary chapter of thirty-eight pages, entitled "*On the Place of Man in the Scale of Being.*"

As regards the plan and objects of this treatise, its author speaks thus:

"In this, as in his previous work, it has been his object to place the reader in the possession of the highest principles that can be regarded as firmly established, in each department of the science; and to explain and illustrate these, by the introduction of as many important facts as could be included within moderate limits. In every instance, he has endeavored to make his statements clear and precise, without being formal or dogmatical; and definite enough to admit of practical application, without appearing to be unimprovable by further inquiry. Physiology is essentially a science of progress; and it must happen that much of what is now regarded as established truth, will need great modification to be brought into accordance with the results of new inquiries. It is very desirable, therefore, that the Student should not be made to think so confidently of his acquirements, as to be indisposed to receive new information, even though it should tend to diminish their value. There are several departments of the science which are, at the present time, in what may be termed a transition-state. Such may be said of the Physiology of Nutrition, which is fast emerging from the deepest obscurity into clearer day; and, as might be expected, there are several questions which can scarcely be at present regarded in any other than a very doubtful light. The Author is not without hope, that, in several parts of his Treatise, suggestions may be found, that will lead some of his readers to original researches on the numerous topics to which they refer; and he considers that he will be thus doing greater service to the public, than if he were himself to aim at embracing the whole range of inquiry thus indicated. With these views, he has not hesitated to express uncertainty where he has himself felt it; and he trusts that what he has thought himself justified in stating with more positiveness, will thus be entitled to greater weight. On one point—the distinct existence of the grey fibres constituting the Organic system of Nerves—he deems it right here to say, that the text (§111) expresses a greater degree of certainty than it would be probably right to assume, on a question at present so controverted. The fact is, that there is so strong a resemblance between these fibres, and those of ordinary fibro-cellular tissue, that it is not easy to distinguish them; and there are many able Microscopists who affirm that they are really identical, whilst others are as positive of their distinctness. To the Author himself, it appears that a strong argument from analogy may be drawn in favor of their existence. In the coats of the arteries and some other structures immediately connected with the Organic Functions, there is a kind of substance resembling ordinary fibrous tissue in appearance, but having many of the properties of the Muscular fabric. It can scarcely, then, be deemed improbable that, in the parts of the Nervous system which are specially destined to influence the Organic processes (if such there be), a similar departure from its regular type, and an approximation towards the ordinary fibrous texture, should manifest itself."

The editorial duties of Dr. Clymer have been performed with discernment, adding to the text of the author whatever the daily improvements in physiology have afforded. Besides, the volume, which is illustrated with numerous engravings, is got up with much neatness.

To such members of the profession as have not the time to peruse, or the means to obtain, the various monographs devoted to these subjects, we would recommend this treatise as well calculated to teach them the

present state of physiology. We will leave to Prof. Caldwell the task, in his present crusade against the chemico-physiologists, of pointing out the heresies of this writer.

ART. XVI.—*The Anatomy, Physiology, Pathology, and Treatment of Cancer.* By WALTER HAYLE WALSHE, M.D., Professor of Pathological Anatomy in University College, London, etc. With additions by J. MASON WARREN, M.D., etc. Boston: William D. Ticknor & Company. 1844. 8vo. pp. 351.

THIS admirable monograph on Cancer, by the distinguished author of the "Physical Diagnosis of Diseases of the Lungs," is reprinted from the Cyclopedia of Surgery; and the publishers, in view of the expensiveness of this cyclopedia, deserve much credit for thus giving it an extensive circulation in this country. As Dr. Walshe has not completed his subject entirely as regards cancer of *particular parts*, reserving these special descriptions to appear under other heads to be published hereafter, the task of supplying the omissions has fallen into the able hands of Dr. J. Mason Warren.

Part. I. treats of cancer in general, its anatomy, physiology, pathology, and treatment.

Part II. is devoted to the disease, as it affects particular parts; such as cancer of the skin, the subcutaneous cellular tissue, the muscles, the subcutaneous glandular parts, the bones, periosteum, and endosteum, the meninges, the alimentary canal, the air-passages, the genital organs of both sexes, the urinary organs and passages, the eye, and the ear.

That this work presents a valuable digest of all that is known upon the subject of which it treats, we have a sufficient guarantee, both in the name of the author and of the American editor.

ART. XVII.—*An Elementary Treatise on Auscultation and Percussion, or the Application of Acoustics to the Diagnosis of Diseases, with a Synoptical Table.* By A. RACIBORSKI, M.D. of the Faculty of Paris, formerly Surgeon in the Polish Army, Professor of Medicine, Chevalier of the Order of the Golden Cross of Poland, etc. Translated, with Notes, etc., by MINTURN POST, M.D. New York: Collins, Keese, & Co. 1839. 8vo. pp. 261.

IN view of the high ratio of the various affections of the chest incident to this climate, it is peculiarly essential that the physician should avail himself of all the means calculated to throw light upon these not unfrequently obscure disorders; but, independent of this additional claim upon American physicians, the day has arrived when to be ignorant of the

advantages to be derived from the application of hearing to the diagnosis of diseases, is unpardonable. Although the physician, from want of sufficient opportunity, may not be able to acquire the tact requisite to detect the obscure and delicate symptoms revealed by their physical signs; yet it is expected from every physician, that he should be at least familiar with the methods of physical examination. It may well be said that the brilliant results which have attended the discovery of the immortal Laennec, in connection with the improvements of Louis, Avenbrugger, Bouillaud, and Piorry, have established a new era in medicine.

The subject of the volume before us is judiciously systematised; and the translator, in the execution of his task, has manifested both good taste and sound judgment.

ART. XVIII.—*Anatomical Atlas, illustrative of the Structure of the Human Body.* By HENRY H. SMITH, M. D., etc. Under the supervision of WILLIAM E. HORNER, M. D., Prof. of Anatomy in the University of Pa, etc. Philadelphia: Lea & Blanchard, 1844.

THIS volume is designed to form a set of plates as an accompaniment to the text of Prof. Horner's work entitled "Special Anatomy and Histology."

"The utility of drawings," says Dr. Smith, "in illustration of a purely demonstrative branch, is now too well established to require any argument in its favor. Separated from the centre of instruction, and deprived of the advantages of the Dissecting Room, the ideas once so thoroughly acquired soon begin to fade, and the images once so distinct, become confused and mixed. A recourse to plates, in the absence of dead bodies, is then the only means of refreshing our knowledge.

"Numerous works, framed with these intentions, have long enjoyed a large share of professional approbation; some from the finished style of their execution; others from some striking feature of simplicity, or adaptation to the wants of medical men.

"The claim of the present one is, to have been selected from the most accurate of these, as well as from the latest Microscopical Observations on the Anatomy of the Tissues; and where plates were not deemed satisfactory, to have been enriched by original drawings, from specimens furnished by the beautiful Anatomical Museum of the University of Pennsylvania."

The portion of the work before us, is Part First, which treats of the bones and ligaments. Of the former there are ninety-two figures, and of the latter, thirty-six. The whole is to be completed in five parts. The work is well got up, alike in reference to the plates, the paper, and the typography. We would recommend it not only to the student, but to the working physician who, although grown rusty in the toils of his harness, still has the desire, and often the necessity, of refreshing his knowledge in this fundamental part of the science of medicine.

ART. XIX.—*A Practical Treatise on the Management and Diseases of Children.* By RICHARD T. EVANSON, M.D., Professor of Medicine in the Royal College of Surgeons, Ireland, and by HENRY MAUNSELL, M.D., Professor of Political Medicine in the Royal College of Surgeons, Ireland. Second American, from the fourth Dublin Edition, with Notes by D. FRANCIS CONDIE, M.D., etc. Philadelphia: Ed. Barington & Geo. D. Haswell. 1843. 8vo. pp. 372.

THIS excellent work has had a fine run at home; and the edition now before us, which is the second one called for, is a reprint of the last Dublin edition. The American editor introduces the volume to his readers, in the following words:

"In presenting a Second American Edition of the Treatise of Drs. Evanson and Maunsell, it may not be improper to state that we consider the work, as revised and enlarged by the authors in their Fourth Edition, to constitute one of the best manuals we possess of the diseases of children, and one especially adapted for the use of the student and young practitioner. It embraces a very full account of the principal affections peculiar to the early period of existence—the phenomena of which are described with great accuracy, while the views advanced in relation to their pathological character, as well as the directions given for their therapeutical management, are evidently the result of extensive personal observations, and a careful study of the views and experience of the best modern authorities."

The contents of this volume embrace the following chapter:

1. Peculiarities of the infant structure and constitution; 2. Management and physical education of children; 3. Mental and moral education; 4. Peculiarities of disease; 5. Infantile therapeutics; 6. Accidents and diseases occurring at birth, or shortly afterwards; 7. Dentition; 8. Diseases of the digestive organs; 9. Diseases of the respiratory organs; 10. Diseases of the cerebral system; 11. Eruptive fevers; 12. Vaccination; 13. Constitutional diseases; 14. Diseases of the skin.

The editor did not deem it necessary to make many additions; but what he has added is characterized by appropriateness. Upon the subject of cholera infantum, for example, he has given an extended note, for the very good reason that his authors have confounded it with the *maladie gastro-intestinale des enfans, avec disorganization gelatiniforme* of the French, and the *gastro-malacia* of the Germans. But this *Irish bull* certainly finds an excuse in the extreme rarity of cholera infantum in Dublin.

ART. XX.—*On the Theory and Practice of Midwifery.* By FLEETWOOD CHURCHILL, M. D., M. R. I. A., Physician to the Western Lying-in-Hospital, Lecturer on Midwifery, etc. With Notes and Additions, by ROBERT M. HUSTON, M. D., etc. With 116 illustrations. 8vo. pp. 519. Philadelphia: Lea and Blanchard. 1843.

THE author, Dr. Churchill, enjoys the advantage of being already favorably known to the profession by his excellent writings on the Diseases of Females; nor does his present treatise detract anything from his well-

earned reputation. Indeed, among the very numerous works, within late years, published on the obstetric art—works, too, rich in practical precepts, and in various modes of illustration calculated to render these rules of service to the inexperienced—Dr. Churchill's treatise maintains a high rank. Embracing a full analysis of the opinions and experience of the many distinguished cultivators of obstetrics, so as to present a faithful exposition of the existing state of the art, the student and the practitioner will both find the volume a useful manual.

The work is divided into three parts, which our author considers as the most natural arrangement.

"The theory and practice of Midwifery," he says, "embraces the anatomy and physiology of the organs of generation, and also the anatomy of the region in which they are contained. A correct knowledge of the structure, magnitude, and other peculiarities of the pelvic cavity is indispensable to a due appreciation of the mechanism of parturition: the anatomy of the organs of generation must of course be preliminary to an investigation into their functions, and it is only by a careful observation of these functions that we are able to detect and understand the deviations from their normal course; in other words, their pathology.

"The three great functions of the uterine system are *menstruation*, *conception*, and *parturition*, which are so intimately connected, that each is dependent on the other, and for the development of either, a co-operation of organs is necessary. A breach of this union, or the absence of this co-operation, will give rise to functional irregularity: and together with the individual deviations, and those arising from organic deficiencies, will constitute the pathology of the sexual system.

"We have thus, in a few words, a natural arrangement of subjects laid down, which we shall follow in the subsequent parts of this volume. PART I. will include the normal and abnormal anatomy of the pelvis, of the external, and of the internal organs of generation. PART II. the function of menstruation, with its abnormal conditions; and of conception, utero-gestation, ovology, &c., with their abnormal deviations, as sterility, superfœtation, extra-uterine gestation, fœtal pathology, abortion, &c. PART III. Midwifery, properly so called,—that is, parturition, with its abnormal varieties.

"This arrangement will bring under our notice all that relates to the theory and practice of midwifery.

"In addition to the description of the different functions noticed above, there will be appended full details for the management, and for the treatment of their deviations; all which I have endeavored to give as clearly, yet as concisely as possible."

One of the most interesting and valuable chapters in the whole volume, is that upon the *premature induction of labor*. Considering the question under the three-fold view of its *morality*, *safety*, and *utility*, the author, after exhibiting a succinct abstract of the opinions, founded upon experience, of all the most prominent authorities, concludes, from an extensive examination, that these three points are fully established. The objections of the French authors he sums up as follows; and these objections, we think, are fairly and satisfactorily answered:

"1. It is immoral.
"2. It is almost impossible to determine the exact relations between the head of the child and the pelvis.
"3. The manœuvres necessary for exciting labor are highly dangerous.
"4. The uncertainty of all women as to the period of their pregnancy.
"5. The difficulty of dilatation of the os uteri at the seventh month.
"6. The danger of subsequent disease."

Dr. Churchill lastly describes five methods of exciting uterine labor, with a view to the induction of premature labor, as recommended by different practitioners.

The addenda of Dr. Huston are always valuable and appropriate.

Art. XXI.—*The Dissector, or Practical and Surgical Anatomy.* By Erasmus Wilson, Author of a "System of Human Anatomy," etc. With 106 illustrations. Modified and re-arranged by Paul B. Goddard, M. D., Demonstrator of Anatomy in the University of Pennsylvania. Philadelphia: Lea & Blanchard, 1844. 12mo. pp. 444.

In this work, we have another valuable aid to the student of practical anatomy. Indeed, so unceasingly does the medical press teem with new productions, in which man's ingenuity is taxed to the utmost, in illustrating by means of figures, what has already been explained in the choicest language, that if there be a royal road to knowledge, it must soon be found. The "Dissector" before us is quite equal to any of its brethren.

Art. XXII.—*Report of a Committee of the Medical Society of Delaware, assigning reasons why the Society should surrender its Charter to the General Assembly.* Wilmington: 1843.

The Medical Society of Delaware, which was originally founded in 1789, aimed merely, as an association of professional men, to extend their knowledge and increase their usefulness. It was not until after thirty years of a purely professional existence that the Medical Society assumed the new character of a censorship, granted by several acts of the General Assembly. These acts—

"Conferred upon the Society the power of interdicting the practice of medicine within the State to incompetent persons, by imposing a penalty of fifty dollars upon any one practising without the license of the Society, and by permitting licenses to be granted only to such persons as produced diplomas conferred by 'some reputable College of Medicine,' or who, otherwise, submitted to a 'full, strict, and impartial examination' by the Board of Examiners, in proof of their medical acquirements. And the acts also imposed a license-fee of ten dollars, to be paid, and appropriated to the uses of the Society."

This power was conferred by the General Assembly, not with the view of promoting the interests of the medical profession, but avowedly for the purpose of protecting the people of the State from the evils of empiricism. But in 1835, a blow was struck at the Society, which somewhat impaired its usefulness:

"By a singular Supplementary Act then passed, it was made lawful for any person to practise medicine without license, and to receive 'any fee or reward therefor, *which might be voluntarily, freely, and gratuitously tendered or given*' to him; but, as it expressly provided that no such person should have 'the right to demand or sue for pay,' the restriction rendered very nearly inoperative a law which, otherwise, would have deprived the Society of all power.

"In the year 1839, the General Assembly, by an act of amendment to the before-mentioned Supplementary Act, expressly repealed the law prohibiting persons practising medicine for pay without license, so far as related to 'persons practising on the *Thompsonian* or *Botanic System* exclusively;' and such persons are, by special provision in the act, authorized 'to sue for and recover fees.' By a further Act, entitled '*An Act for the relief of Homœopathic Physicians*,' passed January 27, 1843, the same exemption from taking out a license, and the same authority to sue for and recover fees, are accorded to 'practitioners on the Homœopathic system exclusively.'

"By these two Acts, it is undeniable that the General Assembly of Delaware have revoked, in every essential feature, the power of the Medical Society to protect the people of the State from the evils of empiricism. It continues the censorship; but it excludes from censure two classes by far the most numerous of empirical practitioners in the United States; and while, by these special acts of partiality, it elevates the two classes into a particular dignity, because into an exemption from laws which apply to regular physicians, and to all the petty classes of empirics who have not yet found favor with the General Assembly, it leaves the Medical Society the appearance of a power which it does not possess, along with the substance of a responsibility, which, in the judgment of your Committee, the people of Delaware will generally believe to attach to the Society, so long as it continues its corporate existence.

Nor is this the only evil resulting from the acts of 1839 and 1843. The laws of incorporation, so far as they are in force, compel licenses to be taken out and paid for by all persons aspiring to practise medicine, *except* those practising exclusively as Thompsonians or Homœopathists. The Legislature, has, therefore, imposed pains and penalties, operating as a direct discouragement, on regular medical men; while it has set a bounty on empiricism. After years of painful study in acquiring knowledge which a hundred generations of the wisest men have slowly contributed to the general stock of medicine, and which, hitherto, has been deemed a necessary acquisition to every Medical man, the young physician, in Delaware, finds himself questioned, and taxed, and licensed, before the laws allow him to enter upon his high calling; while, on the contrary, the most ignorant person, if he but call himself a Thompsonian or Homœopathist, meets with no impediment, pays no tax, is subjected to no question, but commences his occupation, proud of his immunity, and of the ignorance which the General Assembly of Delaware has distinguished as better than the knowledge of the educated physician."

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"The present conjuncture appears to your Committee one in which there is a peculiar call for adopting this dignified course of action, and of surrendering into the hands of the General Assembly the charter of the Medical Society; which, shorn of its usefulness as a public institution, now exists only as a representative of the interests of the Medical Profession. The legislature, as we have seen, has left the Society a shadow of power; but it is a power which exists only to deprive medical men of privileges accorded to Thompsonians and Homœopathists. By the surrender of its charter the Medical Society secures at least equal privileges to the educated physician; and it restores to the public their natural right (the withholding of which is now useless and invidious) of patronizing new and unprivileged classes of empirics, without the necessity of further appeals to the liberality of the Legislature. No advantages have ever inured, or were expected to inure, from the charter of the Medical Society to the Medical Profession, which could not be expected from a private, unincorporated association of Physicians; and by the voluntary relinquishment of the little power still remaining to it, the Medical Society may afford the public a

proof, that the greater power, so long enjoyed by the Society, was not accepted nor exercised from any motives of professional interest, nor for any other purpose than that proposed by the General Assembly in granting it—namely, the protection of the people of the State from the evils of empiricism.

"For these, and for various other reasons which are sufficiently obvious, your Committee are of the opinion, and they beg leave to report accordingly—That it is expedient to surrender the charter of this Society to the General Assembly of the State; and furthermore, That it is expedient, because it will be highly advantageous, to organize a voluntary association for the promotion of Medical and Surgical Science within the State of Delaware."

This report, at the meeting held on the 6th of November last, was unanimously accepted and ordered to be printed; but, as many members were absent, it was resolved to defer the final action until the meeting in May next.

ART. XXIII.—*Lecture, introductory to a Course on the Principles and Practice of Surgery, in the University of Pennsylvania.* Delivered Nov. 6, 1843. By WILLIAM GIBSON, M.D.

WE have perused this lecture with much pleasure, constituting, as it does, a well-digested system for the guidance of the medical student. But as its application is mostly local, our extracts must be limited to a single paragraph:

"It is now well understood, in all colleges of medicine, or ought to be, that almost every course of lectures is susceptible of *demonstration*. Instead of pursuing the old practice of accumulating lecture upon lecture, with a view of exhausting the subject; instead of reading long, learned, and prosing disquisitions, borrowed from old books, detailing theory upon theory, and practice found obsolete and inefficient, the modern lecturer, in all the departments, culls from every source the most precise and accurate information, and presents it to his hearers in so exact and lucid a form, and so much in the shape of a demonstration, as to satisfy them, at once, of the value of what they receive. Now-a-days, no teacher, of any experience or reputation, lectures *largely* upon any subject, belonging, *in common*, to almost every department of the profession, upon which one or more standard writers have published works so valuable, as to receive the decided sanction and approbation of medical men all over the world. Who would venture now, for example, to give an extensive course on Inflammation, after what Hunter and others have published? The same remark will apply to many other subjects. Is it not better, therefore, when the time allotted for lectures is so short, and the subjects so numerous and diversified, instead of attempting, what has never been accomplished—to give *a full course*, as it is called, embracing a little of everything—to devote the greater part of the time to important and difficult topics; such as cannot be well understood merely from poring over books, but which can be explained, readily, by drawings and models and other means, combined with description; and to refer the class, on subjects easily understood, and of less importance, to standard works, which they can read at their leisure, and pursue again and again, until they acquire a full amount of information? No man of common sense will pretend to controvert such a proposition; and each student must see that every moment so precious to him, which is taken up in pursuing an opposite course, cannot fail to rob him of a vast deal of information, only to be obtained from lectures of the true and legitimate stamp."

PART THIRD.

1.

Foreign Medical Intelligence.

ANATOMY AND PHYSIOLOGY.

1. *Anatomy of the Nervous System.*—In the introductory address delivered at the opening of the Medical School of St. Thomas's Hospital, by R. D. Grainger, Esq., F. R. S., Lecturer on Anatomy and Physiology, the following remarks are found:

One of the latest applications of the microscope is that for which we are indebted to Dr. Stilling and Dr. Wallach, and the importance of the inquiry may be judged of by the fact that if the deductions about to be stated are ultimately established, they must considerably modify several of the received opinions on the subject to which they relate. These researches concern the intimate texture of the medulla spinalis and the medulla oblongata, and more especially the origins of the spinal nerves. As there may be some here present who may be desirous of repeating these observations, I will, in the first instance, briefly describe the method adopted by these gentlemen, as given by Dr. Stilling in his work entitled, "Ueber die Textur und Function der Medulla Oblongata." Extremely thin sections of the spinal cord, longitudinal and transverse, are to be made by means of a very sharp and broad razor, rather hollowed on the surface, which, as in making other delicate animal and vegetable sections, is to be kept quite wet on the surface by spirits of wine. These sections, of which there are specimens before us, are made in every part of the cord from the cauda equina to the pons varolii. Considerable importance is attached to the state of the cord when examined; and my own experience has long since taught me that the whole question of success or failure in examining a part of such extreme delicacy very much depends on securing fit specimens. The examination is made most satisfactorily on the spinal cord of an animal, a calf, or dog, for instance, just killed, the delay of a few hours causing considerable changes; but for an extended inquiry like this, it is necessary to have parts always ready, and they are thus prepared:—"I place," says Dr. Stilling, "a fresh spinal cord and medulla oblongata, as they are taken from the body in weak spirit, and allow them to remain in it twenty-four hours; then I pour off the spirit and place fresh but stronger spirit on the parts, which, after two or three days, is again poured off, and then the strongest rectified spirit is used; the specimens being allowed to remain in it from four to eight days, acquire by degrees such hardness as to allow of the finest sections being made."

The sections being provided (if from a recent cord) are then to be extended by means of a compressorium, differing in some respects from the instrument usually employed. Low powers are best adapted for the examination; a lens of two-inch focus being first used, and then others of a higher power. The following is a summary of the facts which are announced in reference to the structure of the spinal cord:—

1. All the white external longitudinal fibres of the cord, anterior, lateral, and posterior, run uninterruptedly from the cauda equina to the pons varolii, and also are continued beyond that body towards the brain.

2. The same course is observed by the longitudinal fibres of the grey internal substance of the cord, and by the fibres of the part named by Rolando substantia gelatinosa, which is placed behind the posterior horn of the grey matter of the cord.

3. The anterior division of the grey matter, but not the posterior, contains a number of corpuscles.

4. A central canal runs along the whole length of the cord.

5. The anterior and posterior transverse commissures of the cord consist partly of transverse and partly of decussating fibres.

The most important observations are those which relate to the connection of the roots of the spinal nerves with the cord; they are as follows:—

1. All the fibres of the anterior and posterior roots pass between the longitudinal fibres of the cord in a transverse and horizontal direction.

2. All the nervous fibres of both roots enter the grey substance of the cord, those of the posterior root previously crossing the substantia gelatinosa.

3. Some of the fibres of the posterior roots become continuous with the fibres of the anterior root of the same side.

4. A part of the fibres of the posterior root cross the mesial plane of the body, passing some before and some behind the central canal of the cord; they then run forward, pass between the corpuscles above noticed of the anterior cornu of the grey matter, and leave the cord as a part of the anterior root of the opposite nerve.

It would lead to details inconsistent with the present occasion to speak of the interesting researches into that most complex organ, the medulla oblongata, respecting the structure of which several parts are described which hitherto were unknown.

To those whose attention has been directed to the intimate texture of the spinal cord, the investigations just noticed are full of interest. Through the kindness of Dr. Stilling, I have had, with many others, an opportunity of examining his preparations. And, in the first place, I would remark that his mode of displaying the disposition of the parts is most satisfactory, and must be regarded as a great step gained in the investigation of the nervous centres. With respect to the facts announced, I am convinced, from repeated inspections, that many of them are strictly true, but not having had an opportunity of examining all the fibres constituting a root of a nerve, I am not prepared to state whether the whole of them, as Dr. Stilling avers, penetrate into the grey matter. The fibres which I did examine undoubtedly passed into that substance; the existence, also, of the central spinal canal, and of the peculiar decussation around it, was plainly demonstrated. This decussation between a part of the posterior root of one side of the body and of the anterior root of the other, is a novel and interesting fact, and will henceforth have to be taken into consideration when the path is investigated by which impressions made on one side of the body, as in physiological experiments and in traumatic tetanus, are transmitted to the opposite.—*Lancet*, *Oct.* 7, 1843.

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2. *On the Presence of Vegetable Germs in the Fluids of Animals.* By MM. Andral and Gavarrett. (*Comptes Rendus des Séances de l'Academie des Sciences*, 30th January, 1843.)—M. Liebig announced in 1841, in a letter to the Academy of Sciences, that he had succeeded in precipitating albumen, in the form of globules, by adding a large quantity of water to serum rendered neutral by an acid. MM. Andral and Gavarrett repeated this experiment, and produced the same result, but assert that these globular bodies, into which the albumen is thrown, are nothing else than the rudiments of vegetables, which M. Turpin has shown are produced in all vegetable infusions on fermentation. After having found these so-called vegetable germs in the serum of blood, they experimented in a similar way on the white of the egg, the different forms of serous fluid produced by diseased action, and the serous part of purulent matter, and met with the same bodies in all these. In every albuminous fluid, when deprived of its alkalinity, they found these globular bodies, which they regarded as vegetable, were produced, and they think that the knowledge of these being vegetable may prove of no small utility in physiology and pathology.—*Edinburgh Medical and Surgical Journal*, October, 1843.

3. *Case of Precocious Puberty.* By M. Ruelle of Cambrai. (*Bulletin de l'Academie Royale de Medecine*, February, 1843.)—The subject of this case was born at Cambrai the 7th of April, 1839, and seen by M. Ruelle a few days after his birth. He was then very small and feeble, and had no strength to take the breast; indeed, it was six weeks after birth before he was able to suck. M. Ruelle lost sight of him till the month of August last, when he was three years and four months old. He had the appearance of a boy about eight years of age. His head was large, yet proportioned to his body. His muscles were well developed; he had twenty teeth widely separated from each other; he was vigorous; had a voracious appetite; and an understanding greatly beyond his years. The down on his cheeks and upper lip was black and very apparent. His penis, when flaccid, was 3½ inches long; the glans was uncovered; the pubis was covered with long black hairs; and the testicles were well developed. The penis became erected whenever it was touched. His mother said he was given to masturbation, which she found it almost impossible to prevent; and that each time a small quantity of seminal fluid was ejected.—*Ibid.*

MEDICAL PATHOLOGY AND PRACTICAL MEDICINE.

4. *On the successful Employment of Belladonna as a prophylactic during the prevalence of Epidemic Scarlatina.* By M. Stievenart of Valenciennes. (*Bulletin de l'Academie Royale de Medecine*, February, 1843.)—An epidemic scarlatina ravaged, during the winter of 1840–1, several villages in the neighborhood of Valenciennes, when Dr. Stievenart was induced to try the prophylactic properties which belladonna is said to possess against this disease. The circumstances rendered any trial of this kind of double value, as, on account of the fatality of the epidemic, 30 patients had already died out of 96 seized. In a small village, out of 250 individuals, 200 took belladonna, and were all preserved from the attacks of scarlet fever. Of the 50 others, 14 were seized with the disease, and four of them died. At the village of Curgies, Dr. Stievenart administered the belladonna to the children at the public school, and allowed them to continue at their lessons and have communication with the other children of the village. All to whom the belladonna was administered escaped the scarlet fever; but a few who refused to take it were seized with the disease.

The belladonna was administered in two forms; in solution, or as a powder. Two grains of the recent alcoholic extract of belladonna were dissolved in an ounce of any aromatic infusion, and of this two drops were given to a child of one year old daily for nine or ten days. An additional drop was given for every additional year of age. The largest daily dose was, however, limited to twelve drops. When the belladonna was given in the form of a powder, half a grain of the powder of the root was mixed with a small quantity of sugar, and divided into ten doses. One of these was given, morning and evening, to children of from one to two years old; two powders, morning and evening, to those from three to five; three powders to those from six to nine; four to those from ten to fourteen; and five to adults.

These small doses never produced the toxicological effects of belladonna; in fact, they scarcely ever had any marked action on the animal economy. In five or six cases Dr. Stievenart observed a rash, similar to that of measles; and, in a few other cases, headache with dilatation of the pupils, dryness of the throat, and slight sore throat, but which had no resemblance to that of *scarlatina anginosa*. In all the others, no sensible or apparent effect resulted from the administration of the remedy.

Dr. Stievenart generally continued the use of the remedy for from nine to ten days; in some cases, it was given for fifteen days. He thinks this period sufficiently long to put the system under the influence of the preservative powers of the remedy; but recommends to return to it if the epidemic return or break out again with renewed violence.

A few of the recorded instances in which belladonna has been successfully administered as a prophylaxis, are noticed. Bayle, in 1830, published a notice

on this subject, by which it appears, that of 2027 individuals to whom belladonna was administered, 1948 were preserved from scarlet fever, and 79 were attacked with it. Dusterberg, by means of belladonna administered for two weeks, preserved from scarlet fever all those who took the medicine. In order to ascertain the real value of the remedy, he purposely omitted to administer it to one child in every family, and this child alone, according to his report, was seized with the disease. He adds, however, that scarlet fever occasionally seized a child who had only been taking the remedy for three or four days, but was always in this case mild, and often only manifested its presence by desquamation ensuing. Zeuch, physician of the Military Hospital for Children in the Tyrol, after eighty-four children were seized with scarlet fever, was induced to try the prophylactic effects of belladonna on the remaining sixty-one children. With a single exception, all were preserved from its attacks, though the disease was raging around. Schenk, Berndt, Kohler, Meglin, De Lens, and other distinguished p hysicians, speak in equally high terms of the prophylactic properties of belladonna.—*Ibid.*

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5. *Atrophy of the Heart in Phthisis.*—Doctor Stokes said the specimens which he wished to present to the Society at that meeting were possessed of considerable interest. They were from the body of a man who had died of phthisis at a very advanced age. The heart presented an appearance which he believed had been remarked previously by only King of London, and R. Adams and R. W. Smith of Dublin. In the present case, there was atrophy to an extreme degree of the heart, a condition already remarked as occurring in chronic phthisis, the heart observing the law of the atrophy of voluntary muscles, but what he particularly desired to direct the attention of the Society to, was the atrophied state of the aortic valves in this specimen ; they were very thin, and in some places cribriform. The filaments corresponding to the perforations were as delicate as a spider's thread. The tongue of this patient was very red, smooth, and dry, but there was no inflammation of the stomach. This appearance of the tongue, usually designated *the beefsteak tongue*, has been also observed in fevers where there was no co-existent gastric affection ; it is, therefore, not to be viewed always as a proof of gastric disease. The cavern in the lung was very large, and contained but little fluid ; as it became dry the metallic sounds became audible ; in the early stage of the disease while there was fluid, there was gurgling, and the sounds could be modified by change of position. When the cavern had become dry, the expectoration ceased, but a little before death it was restored, and this was explained by finding in the opposite lung, two small recently formed cavities. One hydatid was found in the kidney. As during a part of the progress of this case, the cavity in the lungs gave the metallic sounds as observed in pneumothorax, the diagnosis from pneumothorax was based on the signs which indicated the costal and pulmonary pleura to be in opposition. [Proceedings of the Pathological Society of Dublin.] *Dublin Journal of Medical Science*, Nov. 1843.

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6. *Erosion of branches of the Pulmonary Artery traversing Tubercular Cavities.*—Dr. Cathcart Lees said he had to lay before the Society some interesting specimens of disease taken from the bodies of two children, who had died during the week in the South Union Hospital. In both cases there was pulmonary phthisis; in both the immediate cause of death was violent hæmoptysis, and in each case the hæmorrhage came from a similar source. The first case was that of a child three years old, affected with phthisis ; four days ago it was attacked suddenly by profuse hæmoptysis ; this was checked at the time by treatment, but returned after an interval of two days, when the child expired during the attack. On examining the body, the left lung was observed to be filled with tubercles, and contained a large cavity in the supero-posterior portion of its inferior lobe ; into this cavity a large branch of the pulmonary artery could be distinctly traced, from which the fatal hemorrhage had proceeded. At the base of the lung was another tubercular cavity quite distinct from the former ;

across the former cavity there passed several bands which were ascertained not to be pervious; the stomach was distended with blood of a black color and tarry consistence.

The second case was that of a child aged five years, also affected with phthisis, and who, like the other, had been attacked by sudden hæmoptysis, but one day earlier; from that time until its death it complained of constant pain in the side; the hæmoptysis was checked for two days, but returned then, as in the former case, to carry off the patient. After death, when the chest was opened, the left pleura was found full of a sanguineous fluid, in which there were several coagula; the lung was collapsed against the spine; in the inferior lobe superoposteriorly there was a large ragged opening in the lung, communicating with the pleura; this opening was not lined by any membrane; there was round it a whitish line separating it distinctly from the surrounding part, which was flaccid, and of a livid green color; a branch of the pulmonary artery could be traced opening into the side of this cavity. The substance of the lung, as seen in the walls of the cavity, was sloughy and dark-colored, the whole exhibiting the appearance rather of a gangrenous abscess than of a tubercular cavity; both lungs were full of tubercles.

Dr. Lees observed, that the erosion of vessels in the lungs by ulceration was not frequent, but that besides these two cases in which it had occurred, he had been favored by his friend, Mr. R. W. Smith, with another instance of it, in a specimen taken from the body of a lunatic, in whose lung there was found a tubercular cavity, into which a blood-vessel opened in the same manner. Mr. Smith had also met with two other cases during his anatomical investigations, and Dr. W. Stokes had met with a case in which he had been able to transmit air through an artery into a similar cavity. Such cases are, however, of rare occurrence. [Proceedings of the Pathological Society of Dublin.]—*Ibid.*

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7. *Case of Atrophy of the Testicle from excessive Masturbation.* By R. H. Allnatt, M.D., A.M., F.S.A.—A few months ago, a young man, about twenty-two years of age, called upon me for advice. He entered the room with a timid and suspicious air, and appeared to quail like an irresolute maniac when the eye was fixed steadily upon him. In stature he was tall, and he had been evidently well-proportioned, but he was now emaciated, and his hollow eye and shrunken cheek, and dwindled frame, told the tale which he was desirous to withhold. After questioning and cross-questioning him in a variety of ways, and eliciting only indirect replies, I plainly told him my suspicions, when he suddenly started from his seat and exclaimed, "You see before you, Sir, a madman, a wretch, who has as completely committed an act of self-destruction as he who strikes the dagger to his own heart!" I entreated him to be composed and to relate calmly the whole history of his case. He then assured me that from a boy of fourteen he had been addicted to self-pollution, and, according to his account, to an almost incredible extent. The first symptom he noticed was a slight confusion of intellect, which had latterly amounted to a total inability to maintain a connected chain of reasoning. There was dull persistent pain over the brow, pain and weakness in the loins, and a disinclination to the most trivial bodily exertion. He was rapidly approaching a state of complete marasmus.

As I was pondering over the case, endeavoring to fix upon some mild unirritating tonic, he told me, in addition to the ailments he had enumerated, that he was "organically diseased;" and upon asking the nature and situation of this organic lesion he said his left testicle had wasted away, and that his penis had totally lost its erectile power. Upon examination I found that such was literally the fact. The parts were pendulous and flaccid, their virile powers were evidently destroyed, and the left testicle was dwindled to its mere fœtal rudiments, and was almost as yielding to gentle pressure as a piece of moist sponge. Superadded to all, he told me he was "engaged to a very amiable young lady," and he wished my advice how to act under the circumstances I have detailed.

Now, this I call a lamentable picture of human misery, and misery brought about by a species of self-infatuation, an unconquerable propensity, which it

scarcely lies within the province of the medical practitioner to alleviate; for I believe that this wretched young man (and so I told him) on the slightest manifestation of returning power would revert to his vile and solitary habits as surely as the "sow to her wallowing in the mire," and so he thought himself, for so firmly was the vice rooted in him that I could merely extort a hesitating and reluctant assent to a solemn pledge which I entreated him to give of "total abstinence" for the future.

The case, which I have so imperfectly described, was altogether one of the worst and most hopeless instances of self-abuse I have witnessed, and the progressive stages were detailed by the unhappy sufferer with a facility which *experience* alone could produce. At first there were merely transient aberrations of the intellectual functions, which at a subsequent period degenerated into a settled gloom and imbecility. The physical energies, at the onset impaired, became weaker by degrees, and permanently prostrate. The vital affinities of the organ so long subject to unnatural excitement, were destroyed, the molecules entering into its composition were removed by the process of absorption, and, finally, the organ itself, with its elementary tissues, became atrophied in all its constituents.—*Lancet*, August 5th, 1843.

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8. *Advantage of System in Medical Inquiries.*—I advise a systematic arrangement in each case; observe the state of the pulse and skin; feel the head (in insane patients), whether it is hot all over, or in one part only; whether the extremities are cold; whether the tongue is loaded and dry; whether the bowels are open, the urine free, and, if the patient be a female, whether the catamenia are regular. Next observe the breathing and the action of the heart, pass your hand over the right hypochondrium, and feel whether the liver be enlarged, or whether the abdomen be distended with flatus, and whether there be tenderness about the precordia. Examine also the beating of the carotids, and the temporal arteries.—*Lancet*, August 19, 1843.

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9. *Blood diseased in Glanders.* By M. Renault.—M. Renault made a number of experiments to ascertain whether the blood of an animal, laboring under glanders, could communicate the disease to a healthy animal. A horse was inoculated with pus taken from a man laboring under glanders; the horse was soon thereafter seized with the disease. Before killing it a quantity of blood (10 fluid ounces) was taken from its jugular vein, and injected into the veins of a healthy horse. At the end of three days this horse was seized with acute glanders; and blood drawn from it, and injected into the veins of another healthy and vigorous horse, in its turn produced acute glanders also in three days. From these and such-like experiments, M. Renault concludes that the blood is essentially diseased in this fearful malady.—*Ed. Med. and Surg. Jour.* from the *Bul. de l'Acad. de Med.*

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10. *Treatment of Cephalalgia.*—Dr. R. Howard prescribes, with success, for nervous and various other headaches, a mixture of a drachm of acetic acid, an ounce of compound tincture of cardamoms, and four ounces of some convenient vehicle, of which he directs a mouthful to be taken every twenty minutes.—*Lancet.*

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11. *Poultices made with Sea-water.*—We rarely find sea-water used in making flaxseed poultices, notwithstanding the fact that John Hunter often prescribed them; and as he considered, with decided benefit, for buboes and rebellious ulcers existing in scrofulous subjects.—*Annales de Thérapeutique.*

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12. *Remedy for Cough.*—The abrupt violation of the decencies of social existence is one of the most annoying consequences of coughing and sneezing. If

you happen to have a catarrh, and are not endowed with sufficiently strong powers of mind to be able to resist a paroxysm of coughing, place the end of your fore-finger upon the end of your nose, if you have a nose. In the absence of this central organ, rub your eyes as if you had been magnetized, and look at your watch. If the fit of coughing does not pass off, you will at least pass for an accomplished gentleman.—*Med. Chi. Rev.*, October, 1843.

PATHOLOGICAL, THERAPEUTICAL, AND OPERATIVE SURGERY.

13. *Recent Discovery by Mr. Liston, of Spermatozoa, in the fluid of an encysted Hydrocele of the Testis.*—In an appendix to Curling's recent work on the Diseases of the Testis, etc., the author says:

"After Chapter IV., Section 3, which contains an account of encysted hydrocele of the testis, had passed through the press, I was informed by Mr. Liston that on recently examining some rather milky-looking fluid removed by operation in a case of this affection, he had discovered myriads of spermatozoa in it in a lively state, and he has favored me with a view of some of them which were preserved. Mr. L. thinks that this interesting fact may indicate the mode of development of encysted hydrocele; and if further observation should prove that the cyst originates in a dilatation of one of the tubes of the epididymis, that the mucous character of the sac would explain the difficulty which exists in procuring their obliteration by injection. According, then, to this supposition, encysted hydrocele of the testis very much resembles, in its mode of development, the swelling formed by a dilatation of the duct of the submaxillary gland termed *ranula;* but as this view is opposed to that which I have been led to take of the origin of this affection, I availed myself of an early opportunity of carefully examining, in several recent testes, the small serous cysts which are so commonly developed in the head of the epididymis; and in which I concluded, after numerous dissections, that this form of hydrocele commonly originates. I can now state that I have found no reason to question the accuracy of this explanation. In the testis with a single cyst at the head of the epididymis, removed from a man aged twenty-one, who died of fever, abundance of spermatozoa were observed in the contents of the epididymis, but none could be detected in the clear pellucid fluid of the cyst. The cyst was distinctly seen to be unconnected with the seminal ducts; and on examination in the microscope its internal surface exhibited the characters of a serous membrane. I am inclined, therefore, to believe that the occurrence of spermatozoa in Mr. Liston's case was accidental, owing probably to the rupture of the seminal canal, which permitted an escape of the spermatic fluid."

In the Medico-Chirurgical Review, for October, we find the following on the same subject:

"At a late meeting of the Royal Medical and Chirurgical Society, it was announced that Mr. Loyd had found, by the aid of the microscope, numerous spermatozoa in the fluid drawn off by tapping in two cases of common hydrocele. The first case occurred in the early part of last winter. About three months afterwards the second case occurred. The patient was sixty-three years of age, and had been operated on for hydrocele about fifteen times. Sixteen ounces of a highly albuminous greenish-yellow fluid were drawn off. The author counted forty of these animalcules in one drop of this fluid; some of them were observed to retain their power of motion for three hours after the fluid had been evacuated. Blood-globules, transparent cysts, and small granular bodies, with portions of epithelium, or what much resembled it, were likewise found in the fluid.

"At a subsequent meeting of the Society, M. Childs stated that he had met with a third case of hydrocele in the fluid of which an immense number of spermatozoa were present. The fluid was of paler color than that of common hydrocele of the tunica vaginalis, and very much resembled water with which

a very small quantity of milk had been mixed. When tested with nitric acid, and also with heat, the fluid was found to contain a considerable quantity of albumen. There was also much saline matter in it. When examined with the microscope from three to four hours after it was drawn off, there were seen spermatozoa in a living state, and also an immense number that were dead: moreover, the fluid contained a few blood-discs, transparent cysts, granular bodies of different sizes, and epithelial scales. The testis and its appendages were healthy. From the situation and form of the tumor it appeared that the liquid was contained in the tunica vaginalis.

"Mr. Childs had examined the fluid of many hydroceles, but had never met with any animalcules except in these three cases."

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14. *Solvent for Urinary Calculus.*—At the sitting of the Academy of Sciences (Paris), October 9th, a letter was received from M. Millot, stating that he had discovered a solvent for the stone in the bladder. M. Millot does not, like most of the medical men who have directed their attention to the means of dissolving stone, have recourse to drugs which are considered as dissolvents, but which, while they materially derange the general system, lose their power when absorbed, and never produce any sensible effect upon calculi. He recommends local application, and communicates some facts which he regards as conclusive of the efficacy of his plan. M. Millot informs us that having been induced, in the course of some experiments and reasoning by analogy, to suppose that the gastric juice of animals injected into the bladder would dissolve a calculus lodged in that organ, he tried this liquid repeatedly, and always with success. The information is thus far important; but it does not appear that M. Millot has ever tried the gastric juice upon the living organ. His experiments have been performed upon calculi in his own laboratory. Is it certain that the result would be as satisfactory if the experiment were tried upon the living subject? Is there no danger of injury to the coats of the bladder from the presence of the gastric juice? It may be replied that gastric juice is a natural secretion of the stomach, and that it would not be likely to injure any other organ. This, however, is supposition, not fact. Nature may have made the stomach to a certain degree insensible to injurious action, from a secretion elaborated by itself; but would this be the case with another organ, and has it not been asserted that in the stomach itself, where food had long been absent, cases had occurred of very serious lesions by the gastric juice? These were questions put to each other by several of the Academicians when M. Millot's letter was read; but, on the whole, it excited great interest, as a step towards the removal of one of the greatest afflictions to which human nature is exposed. Lithrotity in many cases does much for the relief of the sufferer; but there are others in which it is not at all applicable, and not unfrequently, when it is applied even with success as regards the removal of stone, the derangement of the general system caused by repeated operations leads to serious and even fatal results. A solvent, therefore, which should have the desired effect, without injury to the organ to which it should be applied, would be the greatest blessing of the age.

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15. *Perforating Ulcer of the Appendix Vermiformis.*—Doctor Lees laid before the Society some recent specimens of disease of the intestines in children. The first were from a child four years old, that had died during this week in the South Union Hospital. This child had been suffering from chronic diarrhœa, and was in a state of great emaciation: the discharges from the bowels were intermixed with shreds of lymph, in the membranous form, but there were no sanguineous dejections: there had been incessant vomiting during two days previous to death: the large intestine was very vascular, and all its coats thickened: beneath the mucous membrane were several ecchymosed spots; the vascularity was greatest in the rectum: the lower portion of the ileum also was very vascular, thickened and coated internally with adherent lymph: the coats of the stomach were thickened, and there was a deposition of lymph on its mucous surface, similar to that on the ileum: the mucous membrane itself was of a brown color, and very vascular, presenting an appearance similar to that

called *mamillary* by Louis, and *granular* by Hodgkin. From the pylorus to the ileum, the intestine had no unhealthy appearance.

The next specimens were from a child of fifteen months old, which had been admitted only a week previously, at which time it was described to have been laboring under chronic diarrhœa. On examination of the body after death, the mesenteric glands were found affected by tubercle: the mucous surface of the large intestine was thick, warty, and vascular; but the most remarkable lesion was in the appendix vermiformis, about the middle of which was observed a large patch of green lymph, which covered an ulcer that had nearly perforated into the peritoneal cavity: the mucous membrane appeared to have been removed round a large portion of the tube of the appendix, and in the centre of this ulcerated part, was the minute perforation, which was only prevented from communicating with the sac of the peritoneum by this deposit of bright green lymph; there was no peritonitis. Dr. Lees said that this case was interesting, on account of the early age of the subject, as, although many cases are on record, and the pathological lesion has been fully described, yet the earliest age hitherto mentioned at which it had occurred, was six years, in a case described by Dr. Burne, in his Memoir on Tuphloenteritis, published in the Medico-Chirurgical Transactions.—[Proceedings of the Pathological Society.]—*Dublin Journal of Medical Sciences*, Nov., 1843.

16. *Vegetations on the Semilunar Valves, causing Patency.*—Dr. Stokes said, the specimen which he then produced to the Society, was one of interest in the diagnosis of permanent patency of the aortic valves. The subject of the case had been three months ill, and was subject to difficulty of breathing on taking exercise. There was a double bellows murmur along the sternum, and visible pulsation of the arteries near the surface. The diagnosis was made in accordance with the rules laid down by Dr. Corrigan. When the body was examined after death, the amount of the patency of the aortic valves was discovered to be less than had been anticipated from consideration of all the symptoms during life. The valves were patent to only a slight degree. Water poured into the aorta passed into the ventricle, proving that regurgitation might have happened during life. The imperfection of the valves appeared to be caused by a vegetation on their cardiac aspect. There was also considerable dilatation of the ascending aorta. Might not this, inquired Dr. Stokes, have a similar effect to permanent patency of the valves in producing the throbbing? In both cases there is a cavity and regurgitation into it of a column of the blood. [Proceedings of the Pathological Society.]—*Ibid.*

17. *Detached Bodies found in Bursæ.*—Doctor Hutton said he wished to lay before the Society some specimens of the bodies found in bursæ. They were removed from the upper extremity of a laboring man, a bricklayer, who about a year ago had sprained his wrist. About six months ago he perceived for the first time a tumor which extended from about two inches above the annular ligament of the wrist to the palm. (The appearance it presented could be seen in the cast which Dr. H. then produced.) There was a sense of fluctuation in the tumor. The fingers were contracted, and the index finger still continues incapable of extension. This patient was admitted into the Richmond Hospital on 19th January. The tincture of iodine was tried as a means of promoting absorption, but no effect was obvious on the tumor. It was then determined to operate. On the 2d of February an incision was made into the lower part of the distended bursæ, and the sac emptied of the foreign bodies. Cold water dressing was applied, and the case was progressing favorably (up to the time of Dr. Hutton's communication). In Dupuytren's cases the after treatment was very different; inflammation was excited in the sac by a seton with the view of obliterating the cavity, but the result was frequently a serious inflammation, and even the formation of an abscess in the forearm. The detached bodies produced by Dr. H. were of several shapes. The greater number were oval, but some were triangular. They consist of a matter of the consistence of rice half boiled; the structure is lamellated, the nucleus soft. As to the nature of these

bodies, the opinions are various. Dupuytren considers them to be hydatids or parasitic cysts, but Dr. H. said that he could not by the most careful examination detect any cavity within them, nor any trace of sucking apparatus. Brodie considers them to originate from masses of lymph. Cysts found in the peritoneum were probably of an analogous nature, and these Dr. Hodgkin ascribes to deposits of unorganized lymph, which, after some time, gradually assume a capsular form enveloping some of the fluid of the abdomen. Dr. Hutton inclined to the opinion that these loose bodies owed their origin to effused lymph, that they were increased in size by new deposits, that their shape was modified by the tendons in their neighborhood, and that even within one bursa there might be some of different ages. He was uncertain whether such as were attached by pedicles might not become completely detached, and he mentioned that his experience of these cases showed that a return of the complaint after their removal was by no means unusual. [Proceedings of the Pathological Society.] —*Ibid.*

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18. *Hernia, Hydrocele, and Spermatocele.*—Mr. R. W. Smith presented a specimen of a remarkable combination of morbid states. It consisted of a hernia, a hydrocele, and a spermatocele; the hydrocele lay at the most inferior part, above it was placed the hernia; the enlarged and varicose vessels constituting the spermatocele occupied the posterior part of the hernia and hydrocele, but were largest in the interval between these tumors; the component parts of the cord were widely separated from one another; the vas deferens being placed nearly directly in front of the hernia, but behind the hydrocele; and had the hernia become strangulated, and an operation been rendered necessary, this tube would in all probability have been wounded. [Proceedings of the Pathological Society.]—*Ibid.*

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19. *Orthopedy.*—In a semi-official report of the proceedings of the Scientific Congress of Strasburg, published by one of the vice-presidents, we find it stated that one of the members, Dr. Kuhn, of Paris, formerly an assistant at the institution of la Müeth, asserted that Dr. Jules Guérin had cured five out of nine individuals laboring under abscess by congestion, by his new method of subcutaneous puncture; and that out of 160 patients affected with spinal deviations, the same orthopedist had practised 350 sub-cutaneous sections of the dorsal muscles, which produced, in some instances, *an instantaneous cure;* in others, positive cures with the assistance of consecutive treatment; in others, a decided improvement; and that there were but very few whose condition was not benefited. It would appear, however, from recent observations, that Monsieur Guérin has never accomplished the results that M. Kuhn was commissioned to report to the Congress.—*Annales de Thérapeutique.*

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20. *Gravel removed by Aspiration.*—At a late meeting of the Academy of Sciences, Monsieur Cornay submitted the details of some cases in which he had been able to remove gravel by means of a new instrument of his invention, and through the agency of aspiration. The instrument is called a Lithéréteur.

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21. *Paraphymosis cured by Cold Affusion.*—In 1834 a French soldier was received into the hospital at Nismes for paraphymosis consequent on a blennorrhœa of the most intense kind, and which had lasted for a fortnight. Previously to the man's reception, the regimental surgeon had attempted the reduction of the evil by scarifications, &c., without success, and other ordinary means were adopted by the hospital surgeons to as little purpose. The glans penis was swollen, dark-colored, and acutely painful. The application of cold was now thought of, and made in the following manner:—The patient was seated in a chair, and the operator in front of him, raising the organ affected between his thumb and forefinger. A stream of cold water was now let fall from the height of about a yard, upon the glans, which in a few minutes began to lose its high color, and diminish perceptibly in size; and in five or six minutes from the commencement of the process, the organ was so much smaller that reduction became easy.

Since the period named (says the Journ. de Conn. Med. Chir.) the surgeons of the Nismes hospital have always had recourse to the douche, as above, in cases of paraphymosis, where reduction was unattainable without incision, and in such cases cold affusion applied as above has always proved efficacious.—*Lancet*, August 12th, 1843.

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22. *Removal of Calculi from the Bladder of a Horse.*—We extract the following from the London Medical Gazette for June last, written by Mr. Mogford, V. S.:

My attention has been arrested by an article in your number for January, on lithotomy; a few observations on which, as they are the result of my own experience, will not, I am sure, give offence to that justly respected operator, Mr. Field. I cannot forbear from again expressing my surprise that, in operations of this kind, veterinary surgeons do not make use of the means so peculiarly accessible to them, viz., inverting the bladder through the rectum. Mr. Percivall has very kindly noticed my mode of operation in the third volume of his Lectures and the second of his Pathology.

I first extracted a stone from the bladder in this way in the year 1820, and the case was published by Mr. White in 1824. No operation could be more simple or less exposed to dangerous consequences. There was no inflammatory symptom whatever, and the horse was soon after hunted. In fact, all that is required is a scapula and a probe-pointed bistory, for the arteries are easily avoided without any guide.

In proof of my assertion I may state that I have more than once introduced a stone into the bladder, and extracted it in the same way.

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23. *How to make Leeches Bite.*—The leech which it is intended to apply is to be thrown into a saucer containing fresh beer, and is to be left there till it begins to be quite lively. When it has moved about in the vessel for a few moments, it is to be quickly taken out and applied. This method will rarely disappoint expectation, and even dull leeches, and those which have been used not long before, will do their duty. It will be seen with astonishment how quickly they bite.—*Dublin Med. Press*, from *Weitenweber's Beitr., and Schmidt's Jahrb.*

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24. *Cure of an erectile Tumor of the Face by means of the Vaccine Virus.* By M. Pigeaux. (*L'Experience*, 5th January, 1843.)—The tumor was the common erectile tumor so often observed in infants. It was not noticed at birth, but when the child was about six months old, appeared of about the size of a pin point on the forehead, when it rapidly enlarged to its present dimensions, nearly an inch square, and projecting about three lines above the surface of the adjoining integuments. The tumor nearly disappeared under pressure, but returned the moment the pressure was removed; and whenever the child cried or forcibly exerted itself, it swelled up and became of a deeper red color. It was resolved, on consultation, to try the efficacy of the vaccine virus. The vaccine virus was introduced carefully below the cuticle in nine places over the tumor itself. Very little blood oozed from the slight scratches, but was easily arrested by pressing over the tumor with the point of the finger dipped in fresh vaccine virus. Two scratches were at the same time made over a similar tumor which was situate in the lumbar region.

On the fourth day the vaccine pustule began to show itself. The tumor became of a brown color, and was swollen, and seven small pustules appeared over the tumor, but they remained small till the ninth day. At this period they enlarged much and became confluent. On the twelfth day the crusts formed, and the tumefaction of the erectile tissue was visibly abated. On the twenty-fifth day the crust became detached, when it was found that the surface of the tumor was destroyed, its surface was reduced below the level of the surrounding skin, but the bottom of the wound still contained some red areolar tissue from which blood oozed. This was dressed with powdered alum, and compresses applied, and when the wound healed the tumor was found to have completely disappeared.

The two scratches which had been made on the lumbar tumor had also produced vaccine vesicles, but they had not coalesced, did not include the whole tumor, and when the scales fell off they left a portion of the erectile tumor untouched between them.

From the success of the operation on the tumor on the face, M. Pigeaux recommends that the whole surface of the tumor be, if possible, included in the vaccine inflammation; and, instead of the punctures being made at the circumference of the tumor, that they be made over the whole surface. No portion of the tumor will then escape the inflammatory action, and the disease will stand every chance of being permanently removed.—*Edinburgh Med. and Surg. Journal*, October, 1843.

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25. *On the Employment of Belladonna in the Treatment of Phimosis and Paraphimosis.* By M. De Mignot. (*L'Experience*, 15th December, 1842.)—M. de Mignot having derived benefit from the application of an ointment of belladonna in cases of phimosis and paraphimosis, recommends its employment in every case before having recourse to the knife. The ointment is made in the proportion of 12 grains of the extract of belladonna to 30 grains of simple cerate, and with this the prepuce is rubbed every hour. The dilating power of the belladonna soon begins to act, and in many cases an operation may be avoided. When the inflammation is violent and the pain intense, he recommends to add a little opium and mucilage of quince seeds.—*Ibid.*

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26. *Statistics of Strangulated Hernia treated at Wurtzbourg, from* 1816 *to* 1842 By M. Texton. (*Revue Medico-Chirurgicale de Paris*, January, 1843.)—The number of cases of strangulated hernia treated from 1816 to 1842 was 172. Of these, 98 were inguinal; 71 crural; and 3 umbilical. Of these, two died before they were treated by taxis or operation. Of those treated by the taxis, 105 were cured, and 9 died. Of those subjected to an operation, 32 were cured, and 24 died. Of the 71 crural hernia, 65 occurred in women, and only 6 in men. Of the 98 inguinal hernia, only 13 occurred in women, but 85 in men. It is stated that the average mortality of operations for strangulated hernia at Paris is 4 out of every 7 cases. At Wurtzbourg, it was only 3 out of every 7.—*Ibid.*

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27. *An elegant Sticking Plaster.*—Black silk is strained and brushed over ten or twelve times with the following preparation:—Dissolve ℥ss. of Benzoin in f℥vi of rectified spirit; in a separate vessel dissolve ℥j. of isinglass in water; strain each solution, mix them, and let the mixture rest, so that the grosser parts may subside; when the clear liquor is cold, it will form a jelly, which must be warmed before it is applied to the silk. When the plaster is quite dry, in order to prevent its cracking, it is finished off with a solution of terebinth. chia. ℥iv. in tinct. benzoes f℥vj.—*Medical Times*, *April* 15, 1843.

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28. *On the Local Treatment of Chancres.* By Dr. Strohl.—The more rapidly primary syphilis is cured, the less likely are secondary symptoms to appear. The first object is most easily attained by cauterizing. Sometimes, however, this method is inapplicable, for instance, when the sore is very extensive, or much inflamed. In such cases, the author employs the sulphate of copper. The sores are dressed five or six times a day with charpie, which has been soaked in a solution of about a grain and a half of sulphate of copper to an ounce of water. Simple chancres, when thus treated, usually heal within twelve days. Dr. Strohl assures us, that he cures complicated chancres in an equally short time, with an ointment, composed of two grains of cyanuret of mercury to an ounce of axunge. This ointment is spread upon a piece of linen corresponding to the size of the sore. This dressing is apt to be painful at first, and it must occasionally be taken off, after it has been on for an hour or two, and the remedy must be applied in a weaker form. The pain is said to be most violent in half an hour or an hour, and frequently ceases entirely in two or three hours. When the chancre is extensive and painful, after the ointment has been

on from four to ten hours, according to the sensibility of the patient, it is dressed with mercurial ointment, or opium cerate.

If the edges of the chancre have flattened, if the centre is cleaner, the pain less, and the suppuration healthy (which may occur after the first application of the cyanuret of mercury, but, at any rate, not later than the fourth), the treatment with the sulphate of copper is finished.—*Medical Gazette*, Nov. 11, 1843.

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29. *Neuralgia from Sympathy with a Diseased Tooth.*—A woman, twenty-seven years of age, was accustomed to be awakened at night with a sharp and lancinating pain—in fact, tic douloureux—in the left cheek, which extended to the temple, to behind the ear, and along the anterior border of the trapezius muscle. An hour or two in the morning, during which she slept, appeared to be the sole respite she had from pain. All kinds of rubefacients and narcotics were employed, without any more than momentary benefit, when it was perceived that the second molar tooth was carious, and after some delay it was extracted, when all the pains immediately ceased. Only the enamel of the tooth had been decayed; its roots were sound.—*Gazette des Hôpitaux.*

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30. *On the Use and Abuse of Filing to remove Irregularities of the Teeth.*—There are many instances in which it is advisable and safe, as well as ornamental, to file the teeth; but as it is usually practised, nothing can be more pernicious.

Since it cannot be supposed that any man is so lost to shame and humanity as to expose his patient to pain and inconvenience during life, merely for the sake of a trifling fee, the indiscriminate filing of teeth, so common at present, should be imputed only to ignorance, and may, I hope, be checked, by placing the subject in a clear light, and by drawing the line to distinguish where it may, and where it may not, be practised with safety.

1. In people far advanced in years, the teeth may be filed into order without any inconvenience; because the nerves are lost, the teeth can feel no pain in the operation, nor afterwards from cold, acids, or sweets, and because they are not then so much subject to caries or decay.

2. Where a tooth projects beyond the common level, and hinders the rest from meeting equally, or receives, on itself alone, all the pressure which should fall divided on a whole set, there filing is necessary, at any age, to remove the greater evil.

3. Filing is necessary and advisable to remove sharp points, occasioned by fracture or otherwise, which irritate and wound the lips and tongue; because, in this case, the bony part of the tooth is already exposed, and cutting off the sharp prominences cannot make it more liable to caries or pain than it would otherwise have been.

4. Where a tooth points obliquely against the tongue, or against the lips, as often happens on account of the resistance of the milk-teeth, it is necessary to round the edge by filing, to prevent its wounding the soft parts.

5. When the edges of the fore teeth are uncommonly sharp and thin, and therefore apt to splinter, it is very proper to file them down, to give them a more obtuse and durable edge.

6. Filing is likewise advisable to remove caries, to prepare a tooth for the reception of a new crown, and in a few similar cases, related in the second part of this treatise.

7. When the teeth stand irregularly, and are too broad to admit of being reduced to one uniform line, filing between them, to lessen their size, may be practised to a certain degree; but great care should be taken not to cut away the enamel totally, as is too often practised on this occasion.

If a man had no feeling, nor any other use for his teeth but for the ornament of his countenance, I should not limit the use of the file to these cases only. But since most people, from infancy to middle age, feel insufferable pain the very moment the file touches the bony substance, and since this pain must be very often repeated, because each successive surface of the osseous substance must have some time to wither, and lose a part of its sensibility, before it can

admit of filing beyond a certain depth; since it happens, likewise, that the enamel once removed, is never regenerated; that a tooth in this naked state is for a long time affected with pain from the slightest impressions of cold, acids, sweets, etc.; that it wears away quickly, and is very much subject to decay, I cannot join to support the common practice of indiscriminate filing; I think it should be confined to the cases above related, for I believe it is advisable in these only.

Hence it is, that I so frequently refuse to perform this ornamental operation for my patients, and that I have often advised young people, who have credulously listened to advertisements and promises of this kind, never to barter a sure and valuable blessing for such a painful, dangerous, short-lived ornament: for ill-set or irregular teeth may last healthy and unpained to the latest period of life, and the deformity in general is not very great, provided they are kept clean, white, and polished.—*Berdmore's Treatise on the Disorders and Deformities of the Teeth and Gums.*

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31. *Decadence of a brilliant Operation!*—How, in the name of all that is wonderful, comes it that we never hear now of dividing people's tongues—or, as we heard an elocution professor once tell a public audience, of cutting people's throats—for the cure of stammering? This great achievement of modern surgery has lasted just about as long as a fashionable novel usually does: before a twelvemonth has expired, it is not even talked of. Last year the weekly periodicals teemed with cases of wonderful cures effected "slick off in a twinkling;" and now, alas! there is an utter dearth of anything that excites curiosity. By mere accident we noticed the heading of an article in a late number of the Annales de la Chirurgie—by-the-bye, one of the best of the French journals—on the treatment of stammering by dividing the genio-glossi, and we were curious enough to see what the author would now say upon the defunct subject. With most praiseworthy candor, he tells his readers that, although he met with very decided success in some cases, the result of the operation in his practice, on the whole, was so unsatisfactory that for the last eight months he had entirely given it up. The editor of the journal, in a foot-note, remarks: "It is this conclusion alone that has induced us to insert the memoir of M. *Cloesser*, for it is more remarkable for its good faith, than for any importance of its statements." —*Med. Chir. Rev., Oct.*, 1843.

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32. *Iodine Injections in Hydrocele.*—The use of the tincture of iodine, more or less diluted, in cases of hydrocele as an injection, in lieu of wine, &c., is becoming more and more generally adopted by the French surgeons. M. *Velpeau* has recently written a long memoir, strongly recommending it; and we observe that M. *Pasquier*, surgeon of the Hôtel des Invalides in Paris, reports most favorably of its effects. It seldom produces much pain, even when the injection is strong; and, according to the experience of several surgeons, it seldom or never fails in producing the wished-for adhesive inflammation.

The action of the tincture of iodine on the skin appears to be very similar to that of a strong solution of the nitrate of silver; it stimulates the part for a short time, and then soothes irritation and pain. We have seen good effects from applying it daily to the integuments covering scrofulous and other indolent abscesses. Recently it has been recommended as very useful in strumous, and other kinds of ophthalmia when applied on the outside of the eyelids. It seems to act as a gentle counter-irritant or derivative, in the same manner as the nitrate of silver does.—*Ibid.*

OBSTETRICS AND DISEASES OF FEMALES.

33. *Effects of Menstruation on the Secretion of Milk.—Academy of Medicine.*—M. Raciborski read a memoir on this subject. His conclusions were drawn from seven cases of women who continued to menstruate during the whole or greater part of the period of suckling. They are—

1. That, contrary to the generally received opinion, the milk of nurses who menstruate during the period of suckling does not differ in any appreciable manner from that of nurses who do not menstruate.
2. The only difference worth noting is, that it contains less cream—a fact on which the bluish color of the milk from some women depends.
3. That the inconveniences of allowing a woman to nurse during menstruation, have been greatly exaggerated, and that a nurse should never be rejected on this account alone.—*Provincial Medical Journal.*

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34. *Tannin in Uterine Hemorrhage.*—M. Dumars has published a communication in the Journal of the Society of Practical Medicine, at Montpelier, containing the detail of several cases in which he successfully employed tannin internally in the treatment of uterine hemorrhage. A woman, twenty-five years old, three months advanced in pregnancy, while travelling in a wagon, was seized with a severe colic, which was followed by a frightful uterine hemorrhage. A midwife plugged the vagina, and applied cold lotions to the abdomen and thighs, but without advantage; the plug was expelled. When M. Dumars arrived, the patient was in a state of complete syncope, and death approaching. He ordered pills containing two grains of tannin with a small proportion of opium, one to be taken every hour. By the time she had taken the sixth pill, she fell asleep, and when she awoke, three hours afterwards, the hemorrhage had almost stopped, and had entirely ceased by the next day. The pills were continued every three hours.

In this case the tannin was given in the form of pills; but circumstances may arise during syncope, in which, from the difficulty of swallowing, it may become impossible to administer it in that form. In such cases, M. Dumars prescribes it in the form of draughts or injections. Miss ——, six months pregnant, was seized with violent hemorrhage, after having experienced a severe mental emotion. When seen, she was in a very alarming condition; the abdomen was hard and tympanitic, the skin cold, face pale, the mouth wide open, pulse feeble and intermittent, and complete insensibility. The os uteri was partially dilated, and abortion was very nearly occurring, in fact the slightest delay might prove fatal. M. Dumars ordered four scruples of tannin to be infused for ten minutes in a pint of boiling water, and then to be injected slowly into the bowels. The thighs were brought together, and kept so in a state of flexion, by means of a napkin and pillow, and warm clothes were applied all over the body, after which the following medicine was given, in the dose of a spoonful every half hour: Ninety-six scruples of water of black cherries, sixteen scruples of orange flower water, forty-eight scruples of linden water, ten grains of pure tannin, forty-eight scruples of syrup of diacodium. The medicine was administered by means of a funnel. After the lapse of three hours Miss —— opened her eyes, and the pulse increased in power; some spoonfuls of soup were then given. On examination the hemorrhage was found to have nearly ceased. Twenty-four hours afterwards the patient had recovered her senses; the os uteri had closed, and the bleeding was arrested. The movements of the child were felt soon afterwards. The mother went her full time, and had a happy delivery.—*Ibid.*

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35. *Œdopsophia, or Noisy Expulsion of Gas from the Vagina.*—This affection occurs in women (especially lame women) whose inferior vulvar aperture is narrow. Oftentimes no symptom precedes it, and the patient is not aware of the existence of the complaint until the noisy expulsion occurs. The distended state of the parts may be felt by examination by the rectum, and the gas in part expelled by pressing on the hypogastrium. When the affection arises from a foreign body in a state of putrefaction, it will cease on the removal of this; but in instances where none such existed, Lisfranc has frequently rendered the patient's condition much more supportable, by instructing her, prior to going into society, or, under any circumstance where the unexpected expulsion would prove annoying, to secure herself from this, by introducing a finger into the inferior aperture of the vulva, and thus give vent to it. In examining females for other

complaints, he has frequently detected this affection, although concealed by the patients, and has always succeeded in affording great comfort by the above suggestion. If the vagina is capable of secreting this gaseous substance, it is from its being in a condition of inflammation or irritation, and therefore means of a mild antiphlogistic character, modified by circumstances, must be had recourse to, *e. g.*, venesection, warm baths, lavements, etc.—*Med. Chir. Rev., Oct.*, 1843, from *Lisfranc's Clinique Chirurgicale de l'Hôpital de la Pitie.*

TOXICOLOGY AND MEDICAL JURISPRUDENCE.

36.—*The Poison of Copper eliminated through the Lungs.—Academy of Sciences.* Sitting of 24th July, 1843.—Messieurs Flandin & Danger presented a memoir on the means of detecting the existence of poison in the human body. A curious and interesting fact is communicated in this paper. It has been laid down as a general rule that metallic poisons, administered for some time previous to death, are eliminated from the human system through the agency of the urinary organs. MM. Flandin & Danger assert that this is not the case in poisoning by copper, but that this poison is taken into the lungs, and discharged in the perspiration from the chest, in which secretion it may be easily discovered with proper tests.

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37. *Arsenic administered to Sheep.*—Professor Berreti, of Turin, had recently a public exhibition, at which he proved by experiments upon sheep, that when arsenic is taken in fatal doses, the secretion of urine is *entirely suppressed.—Annales de Thérapeutique.*

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38. *Toxicological effects of Sulphate of Quinine.* By M. Melier. (*Comptes Rendus des Séances de l'Academie des Sciences.*)—M. Melier read a memoir to the Academy, in which he gave an account of some experiments of Magendie on the effects of poisonous doses of sulphate of quinine. Dogs were poisoned with this substance, and on dissection distinct fluidity of the blood was observed, and morbid engorgement of the parenchyma of the lungs. M. Melier, therefore, cautions against administering the large doses which it has been the practice of late to do, and urges its more cautious employment.—*Edinburgh Med. and Surg. Journal*, October, 1843.

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39. *Death from Lucifer Matches.*—Two cases of death have been lately reported from this cause, in England. In one instance, in London, a child, 3½ years old, poisoned herself by sucking the ends of some French Congreve matches. At the inquest, it was stated that they were composed of oxymuriate of potash and phosphorus.

The other case, as quoted in the *London and Edinburgh Monthly Journal of Medical Science*, occurred at Lancaster; and this little patient, aged but six months, also sucked the ends of some lucifer matches.

About eight hours after, violent sickness came on, which continued a night and a day; the matter vomited was luminous, and as well as the breath, emitted a strong phosphorescent odor. After the cessation of the sickness, the child lay in a drowsy, comatose state, until within a few hours of its death, when it was seized with violent convulsions, and died in one of the paroxysms. This occurred on the fifth day. Its bowels during the whole time had been sluggish, requiring enemas constantly in order to cause evacuations.

On dissection, twelve hours after death, a large injected patch was found near the pyloric orifice, and the mucous membrane was thickened and softened. The duodenum and jejunum were healthy, but along the whole course of the ileum,

there were several oblong patches of thickened, softened, and, in many places, ulcerated mucous membrane. The colon and rectum were healthy; the liver pale and somewhat indurated; the lungs and pleura healthy.

40. *Answers returned by the Fifteen Judges to the Questions on the "Plea of Insanity," submitted to them by the House of Lords.*—The answers were read in the name of all the judges, excepting one (Mr. Justice Maule), by Lord Chief Justice Tindal, on the 19th June, 1843.

Question I. What is the law respecting alleged crimes committed by persons afflicted with insane delusion, in respect of one or more particular subjects or persons: as for instance, where at the time of the commission of the alleged crime the accused knew he was acting contrary to law, but did the act complained of with the view, under the influence of some insane delusion, of redressing or avenging some supposed grievance or injury, or of producing some supposed public benefit?

Answer. The opinion of the judges was, that notwithstanding the party committed a wrong act, while laboring under the idea that he was redressing a supposed grievance or injury, or under the impression of obtaining some public or private benefit, he was liable to punishment.

Quest. II. What are the proper questions to be submitted to the jury, when a person alleged to be afflicted with insane delusion, respecting one or more particular subjects or persons, is charged with the commission of a crime, murder for example, and insanity is set up as a defence?

Ans. The jury ought, in all cases, to be told that every man should be considered of sane mind until the contrary were clearly proved in evidence. That before a plea of insanity should be allowed, undoubted evidence ought to be adduced that the accused was of diseased mind, and that at the time he committed the act he was not conscious of right or wrong. This opinion related to every case in which a party was charged with an illegal act, and a plea of insanity was set up. Every person was supposed to know what the law was, and therefore nothing could justify a wrong act except it was clearly proved that the party did not know right from wrong. If that was not satisfactorily proved the accused was liable to punishment; and it was the duty of the judge so to tell the jury when summing up the evidence, accompanied by those remarks and observations which the nature and peculiarities of each case might suggest and require.

Quest. III. In what terms ought the question to be left to the jury as to the prisoner's state of mind at the time when the act was committed?

No answer was returned to this question.

Quest. IV. If a person, under an insane delusion as to existing facts, commits an offence in consequence thereof, is he thereby excused?

Ans. If the delusion were only partial, the party accused was equally liable with a person of sane mind. If the accused killed another in self-defence he would be entitled to an acquittal; but if the crime were committed for any supposed injury, he would then be liable to the punishment awarded by the laws to his crime.

Quest. V. Can a medical man, conversant with the disease of insanity, who never saw the prisoner previously to the trial, but who was present during the whole trial and the examination of all the witnesses, be asked his opinion as to the state of the prisoner's mind at the time of the commission of the alleged crime, or his opinion whether the prisoner was conscious, at the time of doing the act, that he was acting contrary to law? or whether he was laboring under any, and what delusion, at the time?

Ans. The question could not be put in the precise form stated above, for, by doing so, it would be assumed that the facts had been proved. When the facts were proved and admitted, then the question, as one of science, would be generally put to a witness under the circumstances stated in the interrogatory.

Mr. Justice Maule agreed with the judges in respect to the answers returned to all the questions excepting the last; from this he entirely dissented. In his opinion, such questions might be at once put to medical men without deference to the facts proved; and he considered that this had been done, and the legality

of the practice thereby confirmed on the trial of M'Naughten.—*Brit. and For. Med. Rev.*, July, 1843.

MISCELLANEA.

41. *Formulæ used in Guy's Hospital for reporting Cases.*—This is an important subject; for, to obtain reports fit for the purpose of generalization, formulæ are indispensably necessary. It is well remarked by the Editors,* that the history of a case is subordinate to the developing of its present and tangible features. Indeed, little dependence can be placed upon these histories; and when we consider the defects of memory, habits of inaccuracy, and even positive falsehood on the part of the patient, in connection with the possible bias, perhaps involuntary, on the part of the reporter, much uncertainty is thrown upon the results as regards a sound basis for either practical or theoretical conclusions. The following formulæ, however, are as elaborate and complete as can be expected; and we here present them under the belief that, among our readers, there are some who wish to report systematically:

"Their objects have been two-fold—the accuracy, and the number of the reports. And these objects they have essayed to accomplish, by facilitating the duties of reporting: 1st, By the division of labor: and 2dly, By the introduction of formulæ for case-taking. The labors of reporting are of course to be diminished by the number of cases under the charge of each reporter. The whole number of patients in the Hospital at one time is generally about 450—the number of clerks about 30; giving, as a result, about 15 patients as the average complement of each clerk; and, calculating that there are weekly from 55 to 65 admissions, the duration of each patient's residence in the hospital would be about seven weeks. Each clerk would therefore have to report rather more than two fresh cases every week—a number, considered as an average, and consisting of mixed cases, both mild and severe, simple and complicated; evidently in the power of any student, at the requisite period of his studies, to do ample justice to, without neglecting his other subjects.

"The subject of formulæ requires a more lengthy notice. The objects to be aimed at in their formation, are, facility of reporting with uniformity, and increased value of the reports. The first object has certainly been attained, even by the full formulæ now in use by the Society. Indeed many have been induced by them to commence a course of clinical study, the difficulties of which they confessed they should otherwise never have surmounted. So we may pass on to the second item; viz., uniformity and accuracy of the report. In the first place, if cases are to be compared—and such is confessedly one of the main objects of their accumulation—uniformity is almost essential; and in order to uniformity, formulæ are useful, if not positively necessary. These may be full or short, general or special. We believe the fuller they are, the greater is the amount of value accruing both to the reporter and the Society; although, as a point of direct consequence, it must be allowed that the errors would probably be more numerous than in shorter forms, though this only holds good as long as no stride is made beyond the defined limit. The next object contemplated is the fulness of the report, with the accuracy of its details; and for this purpose the formulæ must be full, containing a place for every possible item; and yet so arranged, that the reporter be not left in doubt under what head to note any particular symptom; and that space sufficient be allowed for each head. Special forms, although perhaps of great value in the reduction of matter from a large collected body of reports, imply—so at least it appears to us—a greater advancement in medicine than it has hitherto been our lot to enjoy; for how few diseases can be logically expressed—how widely diseases vary, both as to the number and in-

* Guy's Hospital Reports, for April, 1843.

tensity of the symptoms—and how little hope are we at present justified in entertaining that any law regarding the constancy of symptoms is about to be divulged—though we may perhaps expect some result from the consideration of their variations, and the endeavor to determine their value from their relations."

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"With respect to the construction of formulæ, the rule appears to be concentration of that portion of the report which is usually so indefinite; viz., whatever precedes the admission of the patient, or the first visit of the physician;—with the expansion of that portion of which the reporter can institute his own investigation; viz., the enumeration of the signs and symptoms, suggesting to the physician the name of the disease; which may be usefully considered as a condensed arbitrary expression for the sum of the symptoms.

"The formulæ we at present employ, were suggested by the short forms used by Dr. Conolly, at the Lunatic Asylum at Hanwell; and brought therefrom, in the course of the last summer, by the Honorary Secretary. They have of course been much modified; inasmuch as they are intended for diseases in general, whereas the others are specially designed for one class. They may be divided into several portions, as follows:—

"1. The relations of the patient with respect to the Hospital.

"2. The previous history of the patient.

"3. The causes of the disease, or the link between the previous history of the patient and the present history of the disease.

"4. The history of the present disease, with the date and the order of the succession of symptoms.

"5. The signs and symptoms of the disease, from the sum of which the diagnosis is to be determined.

"6. The history of the progress of the case with the treatment detailed.

"And these again are variously sub-divided, as may be observed by casting the eye over the plan given below.

No. Name of Disease. Result.

Ward — Physician — Date of Admission
No. — Reporter
Name — Single or Married — No. of Children
Age — Occupation

Previous History.
- Abode
- Habits of Life
- General Health
- Previous Diseases or Injuries

Causes or Influence.
- Hereditary
  - Health of Family
  - Causes of Death in Family
- Moral
- Physical

History of Origin, with Date of Present Illness, and Order of Succession of Symptoms.

General Aspect.
- Natural
- Morbid

Development
Position
Integuments and Appendages
Temperature
Moist or Dry
Color
Œdema
Pain or Tenderness
Ulceration or Abscess
Tumors or Eruptions
State of Appendages

Nervous System
Form of Head or Spine
Pain
Vertigo

Lesions of
- Intellect or Disposition
- Motion
- Sensation
- Special Sensation

Respiratory and Circulating System	Digestive System
Form of Chest	Form of Abdomen
Pain	Pain
Voice	Appetite
Respiration	Nausea
Cough	Vomiting
Sputa	Tongue
Results of Percussion	Mouth and Throat
	Defæcation
Results of Auscultation	Dejections
Respiration	Results of Manipulation
	Urino-Genital System
Voice	Micturition
Hearts' Impulse	Urine
Hearts' Rhythm	Generative Functions
Hearts' Sounds	Lesions of Bones or Joints
Pulse and General Circulation	Treatment

Progress of Case	
Treatment	Report

42. *Influence of Season on Sickness.*—Dr. Guy, of King's College Hospital, has published an interesting essay in the last number of the "Statistical Journal" on the influence of the season and weather on sickness and mortality. The conclusions deduced by the author from an examination of the mortality bills and registry kept at King's College Hospital and the Carey-street Dispensary are—

1. The amount of sickness in the central districts of London during the year 1842 varied directly as the temperature; being a maximum in August, the hottest month of the year, and a minimum in January, the coldest month.

2. The diseases which determined the order of sickness were febrile and catarrhal affections, the contagious exanthemata, and the disorders of the digestive organs, to which may be added the mixed group, consisting of gout, scrofula, etc.

3. The diseases of the organs of respiration followed the inverse order of those already mentioned, and were inversely as the temperature, being most numerous in the colder, and fewest in the hotter months.

4. The temperature did not appear to exercise a marked influence on the other classes of disease.

5. The hygrometric state of the air appeared to have little effect on disease, and if it produced any effect it was on the diseases of the organs of respiration, which were in excess during the months in which the quantity of moisture in the air was the greatest, but these were also the coldest months.

6. The mortality for the metropolis during the year 1842, was greatest in the first quarter and least in the second, and was inversely as the sickness, except that the mortality of the third quarter exceeded that of the fourth.

7. The diseases which chiefly influenced the order of the quarters in respect of mortality were those of the chest, to which may be added, as following the same order, the decay of nature in the aged.

8. The order of the seasons in respect of sickness and mortality differs year by year, and does not admit of being reduced to any precise rule.

9. As a general rule, but one admitting of many exceptions, it may be stated

that the amount of sickness tends to vary directly, and the amount of mortality inversely as the temperature.—*Prov. Med. Journal.*

43. *Fork-grinders.*—In an elaborate work on the vital statistics of the town of Sheffield, more especially as regards the influence of the several branches of manufactures, we find the following interesting extract in the Med. Chirg. Rev., for Oct. last:

"This occupation is perhaps more destructive to human life than any pursuit in the United Empire; it exceeds, however, only in a few degrees many other branches of grinding. Fork-grinding is always performed on a dry stone, and in this consists the peculiarly destructive character of the branch. In the room in which it is carried on there are generally from eight to ten persons at work, and the dust which is created, composed of the fine particles of stone and metal, rises in clouds and pervades the atmosphere to which they are confined.

"The dust which is thus every moment inhaled, gradually undermines the vigor of the constitution, and produces permanent disease of the lungs, accompanied by difficulty of breathing, cough, and a wasting of the animal frame, often at the early age of twenty-five. Such is the destructive tendency of the occupation, that grinders in other departments frequently refuse to work in the same room, and many sick clubs have an especial rule against the admission of dry grinders generally, as they would draw largely on the funds from frequent and long-continued sickness.

"The mortality is tremendous. Of 61 persons, engaged in this occupation, 35 died under 30 years of age; 47 under 36 years. Only one attained the age of 48, and he had been fifteen years in the army before entering into this trade.

"To form a proper idea of this mortality, it is necessary to compare it with the ratio of deaths at different periods in other classes.

"Thus in 1,000 deaths of persons above 20 years of age, the proportion between 20 and 29 years, in England and Wales, is annually 160; in Sheffield, 184; but among the fork-grinders, the proportion is the appalling number, 475; so that between these two periods, three in this trade die to one in the kingdom generally.

"Between the ages of 30 and 39, a still greater disparity presents itself. In the kingdom, 136 only in the 1,000 die annually between these two periods. In Sheffield, 164; but in the fork-grinding branch, 410; so that between 20 and 40 years of age, in this trade, 885 perish out of the 1,000; while in the kingdom at large, only 296. Another step in the analysis, and we perceive that between 40 and 49, in the kingdom, 126 die: in this town, 155; and in this branch, 115, which completes the 1,000. They are all killed off. For in carrying forward the inquiry we observe that between 50 and 59, in the kingdom, 127 die; and in Sheffield, 155: but among the fork-grinders, there is not a single individual left. After this period of life, there are remaining in the kingdom, of the 1,000, 441; and in the town, 339; but none in this branch of manufacture.

"In 1841, ninety-seven adult workmen were employed in this trade. Of these ninety-seven, about thirty are at this moment suffering, in various degrees, from the disease peculiar to this occupation, viz., grinder's asthma.

"The disease is seated in the lungs and the air-passages, and the progress of it is accompanied with the gradual disorganization of these important organs. In its advanced stages, it admits neither of cure nor of any material alleviation. In the early stages, the only efficient remedy is the withdrawal from the influence of the exciting cause; but how is this to be effected by men who depend from day to day upon their labor, and whose industry from early life has been confined to one particular branch? Here, then, is the melancholy truth, that nearly one-third of this class of artisans, in addition to the poverty and wretchedness common to the whole, is in a state of actual disease—and disease which no art can cure. Fiction can add no color or touches to a picture like this. Truth transcends the gaudy embellishments of imagination. The distempered fancy has here no room to exercise her powers."

44. *Influence of Climate on Pulmonary Phthisis.—Academy of Science.*—M. Cassimir Broussais read a memoir entitled, "*Note on the Frequency of Pulmonary Phthisis in Different Climates.*" Some years since the Academy addressed to its corresponding members in every country a series of questions relative to the frequency of phthisis, and the proportion of mortality caused by that disease in different climates, but this step failed to elicit the information required, and M. Broussais now endeavors to supply this want. The memoir is based on the documents furnished to the army board of health by the military surgeons from every part of France, and from Algiers. Before investigating the results deduced from those returns, M. Broussais gave a rapid sketch of our previous knowledge on the subject, especially with respect to India, the Antilles, Africa, and Italy.

In Algiers, 62 cases of phthisis occurred among 40,341 patients, or 1 in 650, and but 1 death from phthisis occurred in 102 deaths from all causes; but the average of deaths in the French army from phthisis is 1 in 5.

The difference here shown is so great, and is deduced from such a large number of cases, that it merits the most serious consideration. If the proportions continue the same in future investigations, the favorable influence of the climate of Algiers, on Europeans predisposed to phthisis, will be unquestionably established. M. Broussais admits that serious objections may be made to these results. The troops sent to Algiers may be selected for their health and vigor, and the mortality from phthisis may be replaced by mortality from some other cause, which prematurely carries off the individuals who, had they survived, would have fallen victims to phthisis. M. Broussais terminated his memoir with the following conclusions:

1. No trustworthy documents show that phthisis is common in India.
2. The result of twenty years' observation in the British Antilles establishes that the mortality among the British in those islands from phthisis is generally low, and four times less among the whites than among the negroes.
3. Phthisis appears to be of rather unfrequent occurrence in Martinique, Senegal, Cayenne, and Italy, so far as the imperfect documents respecting these countries admit of any conclusion being deduced respecting them.
4. Phthisis appears to be greatly less frequent in Algiers than in France.—*Dub. Med. Press, June*, 1843.

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45.—*French Gratitude to Medical Men.*—We observe by one of the Paris journals, that the names of *Percy*, *Desgenettes*, and *Larrey*, have been recently engraved on the famous Arc de Triomphe, at the Barrière de l'Etoile.

In some respects certainly, medical men occupy a higher position in society in France than in this country. For example, who ever heard of a doctor becoming a *Baron*, or peer of the realm with us? and yet every one knows that *Portal*, *Dupuytren*, and *Cuvier*, occupied that dignified rank in the parliament of France; and where is the nobleman of Germany that might not be proud to be associated with a Baron *Humboldt?* True, the circumstance of a French peerage being merely personal, and not hereditary, makes a very material difference in the case; but then, why should there not be a certain number at least—we are no friends to an elective aristocracy, as a general question—of the most eminent men in the various departments of science being admitted to the highest honors of the state, without necessarily entailing upon their families the expensive title which they bore themselves?

By a recent Royal Ordonnance, M. *Louis* has been made an officer of the Legion of Honor, and M. *Leuret* a chevalier of the same Order.

Again, MM. *Andral* and *Rayer*—certainly two of the most distinguished names in French medicine of the present age—have been elected members of the Institute. "The selection of such men," says one of the Paris journals, "shows very emphatically that this illustrious body looks as much to the scientific character as to the practical eminence of its candidates; in a word, that it wants not so much the mere skilful physician as the enlightened philosopher in its ranks."—*Medico-Chirurgical Review for July*, 1843.

46.—*Poverty of Medical Men: how to eke out a livelihood.*—"One of the most certain signs (says a writer in the French Medical Gazette) of the 'decadence' of a profession, is the necessity which many of its members experience to seek for supplementary resources. Medicine is a most noble profession, but in the present day it is certainly one of the worst trades (industries) going. It is all very well to talk of dignity and high feelings; let it be remembered that a doctor has a stomach as well as a heart to attend to. In Paris alone, there are not fewer than 2,000 medical men, and it may be readily supposed that a great number of them scarcely ever get a fee. How, then, are they to live until practice comes?—by turning their talents, whatever these may be, to the best account. The other day I called on a confrère, whose circumstances I knew to be in not a very flourishing condition: I found him busily engaged in painting a portrait; he said that he had sold several of late. But what is your object? said I—'renforcer le metier,' was his answer. If a patient called, he put down his palette, doffed his blouse, slipped on his black coat, and with a grave and becomingly professional countenance, went to his consultation-room; no sooner was the interview over, than he returned to his studio, resumed his working dress, and set to his painting again with all the cheerfulness of a brave heart. 'I could not help saying, Apollo is god of the arts as well as of medicine.'

"Several young medical men have exerted themselves with success in the way of 'inventions industrielles;' for example, in the manufacture of portable liquid gas, in the construction of a night-telegraph, in preserving timber for ship-building, &c.

"The lamp C———, we owe to the ingenuity of an intelligent physician, who is practising in the suburbs, and who thus diffuses both light and health around him

"Not a few imitate the example of *Boerhaave*, *Pinel*, and others, and give instructions in languages, mathematics, &c., translating, perhaps, Virgil and Horace in the morning to one set of pupils, and Hippocrates and Celsus in the evening to another. Often the wife, too, lends a helping hand 'à renforcer le metier,' either in the way of teaching, letting lodgings, or perhaps keeping a little magazine of linen, or silk goods, &c.—whatever, in short, brings grist to the mill!"—*Ibid.*

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47. *Comparative Anatomy of the Skin in the different Varieties of the Human Race.*—At the sitting of the *Académie des Sciences* of the 21st August, M. Flourens communicated the results of some of his recent researches upon this interesting question, the subjects of his present investigation being the skins of the Arabs, Moors, Kabyles, and Negroes. As regards the skin of the white man, he had previously established the fact that it is composed of three laminæ or distinct membranes, viz., the dermis and two epiderms, and that there is no appearance of coloring matter between any of these tissues.

In the Arab, Moor, and Kabyle, the color of the skin is a bistre; but this color generally grows lighter in the order in which they here stand. The structure of these skins, however, is the same. Each has a dermis and two epiderms, while between the internal epiderm and derm, contrary to the structure of the white-skinned races, there are found a layer of coloring matter and a pigmental membrane. The same structure of skin is presented by the Negro and the Mulatto, the pigmental membrane and layer of coloring matter being, of course, developed in proportion to the darkness of the complexion.

But the skin of the white man, he shows to be no exception to the law here developed; for it also possesses its apparatus of color, which, though very imperfect, yet is well marked. The skin of the white man at certain points, which are always of a darker shade than the rest of his body, presents all the structure of the skin of the colored varieties of the human race.

This fact, then, affords an additional argument in favor of the original unity of the human race—a doctrine already established on the most incontrovertible basis.

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48. *Inauguration of the Statue of* BICHAT.—This ceremony was recently performed in Bourg, the capital of the department of Ain, which was the native town of BICHAT. The statue, which is executed in bronze by M. David, of Angers, represents BICHAT fixing his gaze on a living infant, while a dead body, partially dissected, lies at his feet; the whole being emblematical of this great physiologist's researches on life and death. To give *éclat* to the event, the aid of processions, music, odes, and orations, was invoked, while the discharging of artillery signalized the first unveiling of the monument to public view. Speeches were successively delivered by the Prefect of the department, the Mayor of Bourg, the Secretary of the French Academy of Medicine, MM. Bonnet, Royer-Collard, Hippolyte Larrey, Brachet, Martin of Lyons, etc.; and these eulogiums have been published at length in the *Gazette des Hôpitaux*. We further observe, that the members of the various medical societies in Paris are desirous of removing the mortal remains of the illustrious BICHAT, at the public expense, from the obscure cemetery of Clancourt to Père la Chaise.

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49. *Anatomical Models.—Academy of Science, July* 3.—Dr. Auzoux, whose anatomical models for the use of Medical students in particular, and for the information of all who take an interest in knowing the functions and uses of every part of the human body, have deservedly attracted so much notice, presented to the Academy some new models in comparative anatomy of extraordinary perfection. Among them is the model of a cockchafer, which, although only twelve times larger than nature, consists of six hundred parts, each marked with its number, and so contrived as to be connected or separated with great facility.

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50. *Professional Honors.*—Dr. Achille Comte, as appears from the *Gazette Medicale de Paris*, has received a gold medal and an autograph letter from the King of Prussia, thanking him for a copy of his beautiful work on Natural History, entitled "le Règne Animal."

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51. *Genius linked to Poverty.*—CHERVIN, whose decease was announced to the French Academy on the 22d of August last, died very poor. The opening passage of his will reads thus: "I have nothing to leave behind me. All that I have inherited or gained by a good practice in Gaudaloupe, has been spent in investigations pursued during a period of twenty-seven years, on the origin and propagation of yellow fever, in order to improve the sanitary regulations with respect to this disease." SIR CHARLES BELL also recently died in a state of poverty. Fortunately, however, as merit is not always without its reward, his widow has been placed on the pension list by Sir Robert Peel, to the amount of £100. These illustrious instances, especially that of CHERVIN, afford additional proof that philanthropy tends to benefit all the world before its possessor.

2.

American Medical Intelligence.

Dr. JARVIS's "SURGICAL ADJUSTER."—This apparatus, which was noticed in the first Number of the Journal, has since then undergone further improvement. Its object, as we then stated, is to reduce all dislocations, to adjust fractures, and to maintain the coaptation of the latter. In a word, it finds a proper application in all cases in which the principle of extension and counter-extension comes in play. As regards its value as a contribution to surgery, Dr. J. has all the evidence that he could possibly desire, in the names of Professors Mott, Parker, and Mütter, Surgeon General Lawson of the Army, Surgeon Ruschenberger, of the Navy, *cum multis aliis*. Indeed, all by whom this instrument has been examined, concur in the opinion, that it fully supplies a highly important desideratum in surgery, and that it will speedily supersede the employment of pulleys, straps, etc., altogether. The surgeon, by means of this instrument, performs his own extension and counter-extension, thus uniting in himself a power capable of tearing a limb from the body; and, at the same time, this power is completely under his own control, nor is he in the least restrained in calling to his aid the manipulations necessary to effect a reduction.

Copious "directions," embracing ten octavo pages, accompany this instrument, in which its multiplied uses are fully detailed; but, in the absence of diagrams, or the apparatus itself, any further description would be scarcely intelligible to the reader.

It is for sale at the drug-store of John Meakim, 511 Broadway.

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Monstrosity.—A most extraordinary vice of conformation has been recently exhibited in a subject brought to Dr. Darling's dissecting-rooms. There is a complete transposition of the thoracic and abdominal organs. The heart is on the right side, while the left lung has three lobes, and the right but two. The liver is on the left side, and the spleen and the sigmoid flexure of the colon on the right. The child was born hydrocephalic, with a complicate hare-lip, and a club-hand and foot. We purpose, in our next Number, to present our readers with a drawing of this curious freak of nature.

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STEWART'S *Lectures on the Diseases of the Genito-Urinary Organs.*—Dr. F. Campbell Stewart, who has given much attention to this subject, proposes to deliver a course of lectures on this class of diseases during the present winter. Having recently returned from Paris, where he spent four years, during two of which he was the private assistant of the celebrated CIVIALE, his instructions upon these affections will, doubtless, prove highly interesting and valuable.

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Velpeau's New Elements of Operative Surgery.—The first volume of this work, which is to appear in February next, from the press of the Langleys, certainly promises to take the lead among all the treatises in this department of medicine. This translation by Dr. P. S. Townsend, is from the Paris edition of 1839; but it also comprises the latest improve-

ments and discoveries in Surgery up to the present time, furnished by Professor Valentine Mott and by Velpeau himself. The work will consist of three volumes, of which the one now in press will be illustrated with two hundred wood-cuts. Velpeau has furnished for this volume from three to five hundred MS. pages of *new* matter. This, in connection with Professor Mott's additions, must surely give to this volume an unrivalled reputation.

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Phreno-Mesmerism, or Neurology.—A letter from a distinguished professor in a Medical College in Kentucky, addressed to a friend in this city, says :

" I have had a letter from Combe, and another from Von Struve, of Manheim, Germany, both of whom are converts to phreno-mesmerism, the cause of which is rapidly advancing, both in Great Britain and continental Europe."—*New York Evening Post.*

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Medical Schools and Students.—As nearly as we can ascertain, there are five hundred students now attending medical lectures in this city, both schools having an increase upon the number of last session.

At Philadelphia, we learn that they have fully their usual quota.

At Louisville, the estimated number is two hundred and fifty.

At Bowdoin College, the lectures in the medical department begin on the 19th of February, and continue three months. The faculty consists of four professors, viz., Drs. Cleveland, Sweetser, Wells, and Peaslee. There are attached to the school a fine medical library and anatomical cabinet.

METEOROLOGICAL OBSERVATIONS.

In accordance with the promise made in our last Number, we now present our readers with the observations of a meteorological observatory, on an extended scale, on Governor's Island, in the harbor of New York. The Medical Society of this city having resolved to institute a similar observatory, it was finally concluded, in view of the great labor connected with such an enterprise, first to make an effort to obtain for this Journal the observations taken in the harbor under the direction of the Medical Bureau of the Army, inasmuch as all the ends aimed at would be thus attained. This request was promptly acceded to by the Surgeon General of the army, notwithstanding it anticipates, in some degree, his regular reports. These observations include the barometer, the thermometer attached and the thermometer detached, the pluviometer, Daniell's hygrometer, the wet bulb, and observations upon the clouds, the clearness of the sky, and the force and direction of the winds. The tables now given comprise four months; and hereafter, each number will contain the intervening two months.

As the meteorological phenomena of every region or locality are modified by its physical features, it follows that a topographical description becomes an indispensable preliminary step in the investigation of its climatic laws. Accordingly, it may be stated that Fort Columbus, on Governor's Island, in the harbor of New York, is in lat. 40° 42′ and long. 74° 2′. This island, situated near the confluence of the Hudson and that arm of Long Island Sound called the East River, is distant from the main, at the nearest point, about half a mile, and from the ocean at Sandy Hook about twenty miles. It is about a mile in circumference,

and is very slightly elevated, with a soil composed of sand, intermixed with fine gravel and loam, based upon ferruginous clay. The portion brought under cultivation yields in abundance the usual esculent vegetables of the climate. There are no swamps or marshes in the vicinity; and in consequence of the regular artificial slope in every direction from the fort to the water, the drainage is perfect.

The following paper, by Thomas G. Mower, M D., Surgeon, United States Army, read at the Centennial Anniversary of the American Philosophical Society, and part of which has appeared in their published "Transactions," will serve to show the present condition of meteorology in our own country:

"This paper, communicated on behalf of the Medical Department of the Army, will present a narrative of the operations of the Bureau in collecting meteorological data; and its further object will be to enlist the co-operation of the lovers of science throughout our country.

"No field of investigation is more inviting to the cultivated and inquiring mind than meteorology, whether we regard the importance, the variety, or the beauty of its objects. The phenomena of the atmosphere attract not only the intellect of the philosopher, but the taste of the painter and the imagination of the poet; and even to the untutored mind, no objects in nature are more pleasing than the arch of the rainbow, the coruscations of the aurora, or the clouds that variegate the summer's sky. To the followers of various avocations, especially to the navigator and husbandman, meteorological pursuits assume an aspect of practical importance; and, in like manner, it was from observing the powerful influence of atmospheric agents in medicine that the Department was led to the investigation of this science. The medical relations of meteorology have been observed by physicians in all ages. It was in order to elucidate and to define these relations, especially as they appear in the medical practice of the Army, that a system of inquiries was instituted, at all our military posts, embracing investigations in regard not only to the number and variety of diseases, their nature, and mode of treatment, but also to the morbific and remedial influences of climate, including the various modifications arising from local position and other physical agents. Meteorology was thus made the object of observation, less as an independent pursuit than as it is subservient to a superior design—the advancement of medical science. It is intended, however, that the labors of the Department should be so conducted as to be available also for the purposes of science in general.

"The duties required of an Army Surgeon are so numerous, that only a small portion of his time and attention can be devoted to collateral branches of science. Hence, in meteorology, all that can with propriety be expected of him is, that he be a diligent collector of facts. As in war, an army is the protector of science, so in peace it may become a useful adjuvant. Both here and in Europe, the military organization has been used as a channel of scientific inquiry—a channel, in consequence of the harmony and extent of its operations, of no small value.

"The advantages of *system* and *co-operation* in meteorology are peculiarly great; and the Royal Society of London, in their meteorological directions, say truly, 'that systematic co-operation is the essential point to which at present everything else should be sacrificed; and co-operation on almost any plan would most certainly be followed by more beneficial results than any number of independent observations, however perfect they might be in themselves.' Were the cultivators of meteorology throughout the United States, in connection with State institutions, to unite in a uniform plan of operations, more progress may be expected, within a short period, toward the perfection of the science, than has yet been accomplished by their isolated efforts.

"Previous to the year 1818, we possess no records of meteorological observations, taken in the United States, on an extensive scale. During that year, the surgeons at the military posts were directed to keep regular records of the

weather, and transmit them quarterly to the Medical Bureau at Washington. The earliest registers thus transmitted, and on file in the Surgeon General's office, are dated January, 1819.

"The institution of meteorological observations and the organization of the Medical Department, were nearly cotemporaneous. By an act of Congress, passed April, 1818, upon the enlightened and liberal recommendation of the Hon. John C. Calhoun, then Secretary of War, a Surgeon General was appointed, who was charged with the administrative details of the medical staff. Inquiries into meteorology were begun the same year; and thus did the Department, at its very origin, enter on a course of investigation, in which it has since been followed by the States of New York, Pennsylvania, and Ohio. New York commenced in 1825, Pennsylvania in 1836, and Ohio during the last year, 1842.

"'The merit of introducing meteorological observations into the army is due to the late Surgeon General, Doctor Joseph Lovell. Soon after his appointment, he submitted to the Secretary of War for approval, a system of regulations for the government of the Department. Therein, among other duties, the surgeon is required 'to keep a diary of the weather,' and 'to note everything relating to the local situation of his hospital, the climate, diseases prevalent in the vicinity, etc., that may be calculated to produce or vary the forms of disease.' 'Every physician,' Dr. Lovell remarks, 'who makes a science of his profession, or aims at eminence in it, will keep a journal of this nature, as the influence of weather and climate upon diseases, especially epidemic, is perfectly well known. From the circumstances of the soldier, their effects upon diseases of the army are peculiarly interesting, as, by proper management, they may in a great measure be obviated. To this end, every surgeon should be furnished with a good thermometer, and in addition to a diary of the weather, should note everything relative to the topography of his station, the climate, complaints prevalent in the vicinity, etc., etc., that may tend to discover the causes of disease, to the promotion of health and the improvement of medical science.'

"The only instruments used at the first were a thermometer and vane, and the observations were confined to the temperature of the air and the course of the winds, with general notices of the weather. The thermometer was noted at 7 A.M., 2 P.M., and 9 P.M. (the hours now used by the Franklin Institute), and the winds and weather were observed morning, noon, and evening. Early in the year 1836, a rain gauge was furnished to each post, and the daily fall of rain added to the register. These observations have been continued without material change, for a period of twenty-four years, to the commencement of the present year, and without interruption, except such as has arisen from our military operations against the Indians, and more especially from the late war in Florida.

"The results of the observations for 1820 and 1821 were published at the end of each year. In 1840, an 'Army Meteorological Register' was published, embracing the years from 1822 to 1830 inclusive. It is the purpose of the department shortly to give to the public the entire observations up to the present period.

"The principal object of observation has been the temperature of the atmosphere; and while it is to be regretted that the objects were so few, still, as temperature is a prominent property of the air, and, perhaps, the most influential element in the formation of climate, it is hoped that the investigations on this point are not without value.

"As points of observation, our military posts possess peculiar advantages. They extend from the 25th to the 46th degree of latitude, and from the 67th to the 96th degree of longitude—an immense region, embracing the whole inhabited area of the United States. A large proportion of them stand at intervals, along two nearly parallel lines running from south to north, and forming our eastern and western frontier, extending on the Atlantic from Key West, near the Bahamas, to Passamaquoddy Bay, and on the west, from the Balize to the Falls of St. Anthony. From this disposition of our posts, it happens that almost every parallel of latitude intersecting our country passes through a military station; and, as the mean annual temperature of these posts has been

determined by a series of observations extending from ten to twenty years, we have been enabled to draw isothermal lines, coinciding, at one or more points, with almost every parallel of latitude between Cape Sable and the St. Lawrence. The distribution of temperature among the seasons and the different months of the year, by which so important an influence upon vegetable geography is exerted, has also been very generally determined.

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"The limits of this paper will not permit of a detailed statement of the many interesting facts which may be deduced from the records of the Department. A general view of the climatic features of our country has lately been presented to the public in a work entitled 'The Climate of the United States, and its Endemic Influences,' written by Samuel Forry, M.D., of New York, formerly of the army, who was then employed, under the direction of the Surgeon General, to collate and condense for publication, the meteorological matter collected in the Medical Bureau. This work, based on the 'Army Meteorological Register,' and 'Medical Statistics,' has attracted the favorable notice of distinguished men at home and abroad.

"Within the last few years, the attention of the Department has been awakened afresh to the subject of meteorology. Under the direction of the present Surgeon General, Dr. Thomas Lawson, the observations have been greatly extended, and the various stations have been supplied with the requisite instruments. In May, 1842, the Army Medical Board, then in session at Philadelphia, was instructed by the head of the Department, to prepare practical directions for taking meteorological observations in an extended form. The Board, consisting of Surgeons Mower and Stinnecke, and Assistant Surgeon Cuyler, diligently sought information from all authentic sources within their reach, and at length drew up a set of directions, which were submitted for the approval of the Surgeon General and Secretary of War, and finally adopted for the government of the medical officers, in their meteorological observations. In preparing these directions, the Board received valuable aid from Professor A. D. Bache, Professor Bartlett of the U. S. Military Academy, J. P. Espy, Esq., and from the instructions of the Regents of the University of New York, of the Franklin Institute at Philadelphia, and of the Royal Society, London.

"The new form went into operation on the first day of January, 1843. The Department, in selecting objects for observation, have endeavored to render their registers as uniform as may be with the journals of the various scientific institutions in this country now engaged in similar pursuits. The preparation of a perfect system has not been attempted, because in the present state of meteorological science, such an attempt would certainly fail of success. Every system, in the mean time, must be considered as merely experimental. But it is believed that the plan now introduced into the army, will, in the end, furnish the means for its own correction and improvement.

"Observations are now taken on the thermometer, barometer, attached thermometer, wind, clouds, clearness of the sky, and wet bulb. These observations are recorded at four different periods during the day, except on the wet bulb which is taken only twice. The time when rain began and ended, and the quantity which fell, are noted at the close of every shower. In addition, the medical officers are requested to note, under the head of 'remarks,' all remarkable phenomena, especially sudden and simultaneous changes of wind and temperature; their effect on the barometer; the moment of greatest depression of the barometer in the passage of storms; the time of clouding; currents of clouds moving in different directions, and at different heights; the rise and fall of rivers and lakes; remarkable tides; the opening and closing of navigable waters; the last killing frost that occurs in spring and the first in autumn, as shown by their effects on the tender buds, leaves, and germs of fruit trees, etc.; the commencement and progress of vegetation; the first appearance and departure of birds of passage; thunder storms, near or remote; silent lightning with its direction, and elevation above the horizon; falls of hail, snow, and sleet; fogs; white or hoar frosts, etc.; also, to examine the heavens at the latest hour of observation, whether there be any aurora or shooting-stars; and especially about the 10th of August,

and 12th and 13th of November, to see whether there be any great number of luminous meteors visible, stating the number observed in an hour, or at least in a quarter of an hour; and further, in cases of great fires, occurring in clear, calm, dry weather, with a high dew point, to observe whether clouds form over the fire, and to describe the phenomena.

"At the equinoxes and solstices, hourly observations of the barometer are directed to be taken for twenty-four hours, to correspond with those already instituted at numerous points of Europe and America, at the suggestion of Sir John Herschel. The days fixed upon for these observations are, the 21st of March, June, September, and December. But should any one of these twenty-first days fall on Sunday, then the observations will be deferred till the twenty-second. The observations at each station will commence at six o'clock, A. M., and be continued at the beginning of each hour till six o'clock A. M., of the following day, care being taken to obtain the correct time.

"A thermometer on a new pattern, finely adapted for observing the wet-bulb temperature, has been contrived for the Department by Geo. Tagliabue, philosophical instrument maker, New York. The barometer employed is Bunten's Syphon, imported from Paris, on the recommendation of Capt. Talcott, U. S. Commissioner for surveying the North Eastern Boundary. Its advantages are sensitiveness, accuracy, portableness, and cheapness. A number of Daniell's Hygrometers were received from London. They are beautiful instruments, compact, simple, and portable. But the principle upon which they are constructed answers better for the humid climate of England than the arid atmosphere of the United States. The experience of Prof. A. D. Bache, and experiments made at several posts, induced the Department to prefer the wet-bulb thermometer in determining the hygrometric condition of the air. The conical rain gauge, originally introduced into the academies under the direction of the Regents of the University of New York, by Simeon De Witt, Chancellor of the University, has been adopted. A full description of this instrument, and the principle upon which it is constructed, will be found in Silliman's Journal for June, 1832.

"The periods recommended for meteorological hours by the Royal Society, London, are 3 and 9 A.M., and 3 and 9 P.M. These periods the Department has adopted, with the substitution of sunrise for 3 A.M., as being more seasonable, and better suited to the routine of military service. The maxima and minima of the barometric oscillations, at the level of the sea, probably occur at these hours over a large portion of the globe. Besides, the lowest degrees of temperature and dewpoint are obtained, as experience has shown, shortly before the dawning of day, and the highest degrees nearly at 3 P.M. The pair of hours, 9 A.M. and 9 P.M., coincide nearly not only with the maxima of atmospheric pressure, but with the periods of mean morning and evening temperature, and half the sum of these two observations will be a near equivalent for the mean daily temperature, as is shown by Capt. Mordecai, of the corps of Engineers, in his observations at Frankford Arsenal, near Philadelphia.

"Observations are now made at sixty military stations, which, with few exceptions, are situated either on our maritime or inland frontier. The following list will give their distribution through the different States and Territories. Five are in Maine, one in each of the States of New Hampshire, Massachusetts, Rhode Island, and Connecticut, nine in New York, four in Pennsylvania, two in Maryland, one in Virginia, two in North Carolina, one in South Carolina, two in Georgia, four in Florida, two in Alabama, five in Louisiana, three in Arkansas, one in Arkansas territory, three in Missouri, one in Missouri territory, four in Iowa, two in Wiskonsin, and five in Michigan.

"Barometers have been furnished to twenty-five of these posts, or wherever there was a prospect that the post would be permanently occupied; nine are placed on the Atlantic, including the extreme northern and southern points, three on the Gulf of Mexico, and six on the Western frontier. On the great lakes, which exert so marked an influence on the climate of the adjacent regions, the barometrical sites are Sacket's Harbor, at their eastern extremity; Buffalo, between Erie and Ontario; Detroit, nearly at their most southerly stretch; and Fort Brady, further north, at the outlet of lake Superior. Plattsburgh, on lake Champlain, is also supplied.

"But while, from the position of our posts, the points of observation are thus extended along the frontier, the *interior* of our land is left comparatively destitute. On the Atlantic slope, only two barometers are placed at any considerable distance from the ocean, viz., at Carlisle barracks, Pennsylvania, and Augusta Arsenal, Georgia; and there is not a single point of observation in the large tract of territory embracing the States of Ohio, Indiana, Illinois, Kentucky, Tennessee, and Mississippi.

"The Department would, therefore, cordially invite the co-operation of colleges, scientific institutions, and individual admirers of meteorology, in these sections of our country. The directions and forms used in the Army, may at any time be obtained, by application, addressed to the Surgeon General, U. S. Army, Washington. All contributions from other sources will be cheerfully acknowledged in the publications of the Department. Under the direction of the Surgeon General, arrangements have been made for prosecuting meteorological inquiries with renewed diligence; and a medical officer will shortly be detailed to give his chief time and attention to the subject, to arrange and digest the matter collected, and to prepare the results for publication.

"*May* 23, 1843."

To render the following meteorological tables completely intelligible, some further explanation may not be amiss. The barometer employed is Bunten's syphon, the scale of which has of course the French division of *centimetres*. The table on the next page will, however, enable any one to reduce the French barometer scale to the English:

In the "*thermometer attached*," the centigrade scale is used, in which the point of melting ice is 0, and that of boiling water is 100°. In reference to the scale of Fahrenheit, degrees may be converted from one to the other by the following formula: To reduce a degree of centigrade to that of Fahrenheit, multiply by 9-5ths, and then add 32, and *vice versâ*.

The third division is the "*thermometer detached*," which is according to Fahrenheit's scale.

The degree of "*clearness of the sky*" is designated by the figures 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10. The first (0) signifies no clear sky; and so on to 10, by which entire clearness is indicated.

Under the head of "*wind*," both its direction and estimated force are registered. The letters denoting the points of the compass are of course understood as indicating the *direction* of the wind, while its *force* is expressed by figures, from 0 to 10; thus 0 signifies a calm; 1, a barely perceptible breeze; 2, a gentle breeze; 3, a moderate breeze; 4, a brisk breeze; 5, a strong wind; 6, a very strong wind; 7, a storm; 8, a great storm; 9, a hurricane; and 10, a violent hurricane.

Under the head of "*clouds*," the same expressions are used as in registering the wind; and it is only the direction and velocity of the lowest stratum, when several strata or currents of clouds exist at different altitudes, that are noted.

The temperature indicated by the "*wet bulb*," is according to the scale of Fahrenheit.

The amount of rain which falls is measured by the conical rain-gauge, which expresses the quantity in inches and decimals.

The dew-point indicated by Daniell's hygrometer, is expressed according to Fahrenheit's scale.

Hereafter, we may, from time to time, lay before our readers some of the general laws of meteorology, developed throughout the United States by this extended system of observation.

U of M

1 *French mètre* = 39,371 *English inches.*

Centimetres.	English Inches.	Centimetres.	English Inches	Centimetres.	English Inches.	Centimetres.	English Inches.	Centimetres.	English Inches.	Centimetres.	English Inches.
68·60	27·01	70·80	27·87	73·00	28·74	75·10	29·57	77·20	30·39	79·30	31·22
68·70	27·04	70·90	27·91	73·10	28·78	75·20	29·61	77·30	30·43	79·40	31·26
68·80	27·08	71·00	27·95	73·20	28·82	75·30	29·65	77·40	30·47	79·50	31·30
68·90	27·12	71·10	27·99	73·30	28·86	75·40	29·69	77·50	30·51	79·60	31·34
69·00	27·16	71·20	28·03	73·40	28·90	75·50	29·73	77.60	30·55	79·70	31·38
69·10	27·20	71·30	28·07	73·50	28·94	75·60	29·77	77·70	30·59	79·80	31·42
69·20	27·24	71·40	28·11	73·60	28·98	75·70	29·81	77·80	30·63	79·90	31·46
69·30	27·28	71·50	28·15	73·70	29·02	75·80	29·85	77·90	30·67	80·00	31·50
69·40	27·32	71·60	28·19	73·80	29·06	75·90	29·89	78·00	30·71	80·10	31·54
69·50	27·36	71·70	28·23	73·90	29·10	76·00	29·92	78·10	30·75	80·20	31·58
69·60	27·40	71·80	28·27	74·00	29·13	76·10	29·96	78·20	30·79	80·30	31·62
69·70	27·44	71·90	28·31	74·10	29·17	76·20	30·00	78·30	30·83	80·40	31·65
69·80	27·48	72·00	28·35	74·20	29·21	76·30	30·04	78·40	30·87	80·50	31·69
69·90	27·52	72·10	28·38	74·30	29·25	76·40	30·08	78·50	30·91	80·60	31·73
70·00	27·56	72·20	28·42	74·40	29·29	76·50	30·12	78·60	30·95	80·70	31·77
70·10	27·60	72·30	28·46	74·50	29·33	76·60	30·16	78·70	30·98	80·80	31·81
70·20	27·64	72·40	28·50	74·60	29·37	76·70	30·20	78·80	31·02	80·90	31·85
70·30	27·68	72·50	28·54	74·70	29·41	76·80	30·24	78·90	31·06	81·00	31·89
70·40	27·72	72·60	28·58	74·80	29·45	76·90	30·28	79·00	31·10	81·10	31·93
70·50	27·76	72·70	28·62	74·90	29·49	77·00	30·31	79·10	31·14	81·20	31·97
70·60	27·80	72·80	28·66	75·00	29·53	77·10	30·35	79·20	31·18	81·30	32·01
70·70	27·84	72·90	28·70								

METEOROLOGICAL

STATION, Fort Columbus, New York Harbor. Latitude 40° 42′ North. Longitude 74° 9′ West Altitude of Baro

1843.	BAROMETER.				THERMOMETER ATTACHED.				THERMOMETER DETACHED.					CLEARNESS OF THE SKY.				W	
August.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 A. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily Mean.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M
	Centi-metres.	Centi-metres.	Centi-metres.	Centi-metres.	Centi-grade.	Centi-grade.	Centi-grade.	Centi-grade.	Fahr.	Fahr.	Fahr.	Fahr.	Fahr.						
1	76·30	76·40	76·40	76·25	19	21°	26°	20°	66°	70°	89°	70°	74°	4	6	3	8	NE 1	N
2	76·40	76·43	76·50	76·50	17	21	27	18	64	70	81	70	72·5	0	5	10	10	NW 0	NE
3	76·65	76·70	76·70	76·80	20	23	26	19	68	74	80	70	74	0	8	10	10	NE 0	NE
4	77·12	77·25	77·25	77·20	14	22·5	27	22	65	72	82	75	73·5	6	9	10	10	NE 0	NE
5	77·15	77·15	77·00	77·00	20	23	25	24	70	74	74	72	72	0	4	0	0	NE 1	NE
6	76·95	76·80	76·80	76·80	18	21	22	20	66	68	70	68	68	5	9	0	0	NW 3	NW
7	76·95	76·90	76·90	76·80	17	21	24	19	65	69	78	66	71·5	0	0	6	0	E 0	SE
8	76·52	96·48	76·40	76·40	22	23	28	23	72	74	80	72	76	0	0	0	0	SE 1	SW
9	76·40	76·40	76·40	76·50	20	21	23	19	69	70	74	70	71·5	0	0	0	10	NE 1	NE
10	76·60	76·68	76·40	76·00	20	22	23	20	69	74	72	68	70·5	6	5	0	0	NE 0	NE
11	76·10	76·20	76·22	76·25	20	23	24	19	68	74	75	70	71·5	0	0	6	0	NW 2	NW
12	76·20	76·30	76·40	76·30	17	21·5	28	21	65	71	84	72	74·5	8	10	10	10	NW 1	NW
13	76·30	76·42	76·45	76·40	17	21	31	23	68	71	90	75	79	10	10	10	10	W 1	NW
14	76·40	76·40	76·05	76·05	21	24	26	22	71	76	81	74	76	10	10	10	10	NW 1	N
15	76·00	76·00	76·10	76·05	20	23	30	20	70	74	88	71	79	10	10	10	10	NE 1	NE
16	76·00	76·00	76·00	76·00	18	21	31	24	65	71	82	74	73·5	10	10	10	10	W 1	NW
17	76·00	76·00	76·05	75·90	19	21	30	21	68	74	80	75	74	10	10	10	10	SW 1	SW
18	75·90	76·00	76·00	76·00	20	24	29	24	70	76	82	75	76	0	10	10	10	S 1	SW
19	76·00	76·00	76·00	75·95	22	25	28	22	72	77	80	73	76	10	10	10	6	SW 1	NW
20	75·80	75·80	75·70	75·70	20	21	26	21	70	73	79	72	74·5	0	5	5	0	NW 0	NW
21	75·85	76·20	77·00	77·00	19	23	28	20	69	74	83	70	76	0	6	6	0	NW 1	NE
22	77·00	77·00	77·00	77·00	19	20	53	23	68	69	72	73	70	0	0	8	0	NW 0	NW
23	76·70	76·72	76·70	76·65	20	24	26	24	71	74	76	71	74·5	0	6	8	0	NE 1	NE
24	76·68	76·75	76·70	76·70	20	22	30	24	70	73	80	74	75	0	5	10	0	NE 1	NW
25	76·75	76·80	76 80	76·75	20	23	31	25	70	74	86	75	[illegible]	10	10	10	10	N 0	NE
26	76·70	76·70	76·70	76·85	19	24	26	23	69	76	79	75	[illegible]	10	10	10	10	SW 0	W
27	76·80	76·90	77·00	77·00	20	28	28	24	69	76	86	76	77·5	5	5	8	0	W 0	W
28	76·90	76·90	76·90	76·90	23	24	26	23	73	76	80	75	76·5	0	0	5	0	SW 1	SW
29	76·95	76·95	77·00	77·00	21	22	24	20	70	73	74	72	72	0	0	5	0	NE 1	E
30	77·00	77·00	77·00	77·00	20	24	26	24	70	74	79	74	74·5	0	6	8	0	NW 1	W
31	76·80	76·80	76·90	76·80	21	25	31	24	70	76	86	72	78	5	10	10	0	NW 1	NW
Monthly Mean.	76·51	76·55	76·56	76·53	19·45	22·65	27·20	21·7	68.71	73·10	79·83	72·23	74·29	3·84	5·84	7	4·65		

METEOROLOGICAL

or. Latitude 40° 42′ North. Longitude 74° 9′ West Altitude of Baro

	THERMOMETER DETACHED.					CLEARNESS OF THE SKY.					
	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily Mean.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A.
	Fahr.	Fahr.	Fahr.	Fahr.	Fahr.						
	66°	70°	82°	70°	74°	4	6	3	8	NE 1	N
	64	70	81	70	72·5	0	6	10	10	NW 0	NE
	68	74	80	70	74	0	8	10	10	NE 0	NE
	65	72	82	75	73·5	6	9	10	10	NE 0	NE
	70	74	74	73	72	0	4	0	0	NE 1	NE
	66	68	70	68	68	5	0	0	0	NW 3	NW
	65	69	78	66	71·5	0	0	6	0	E 0	SE
	72	74	80	73	76	0	0	0	0	SE 1	SW
	69	70	74	70	71·5	0	0	0	10	NE 1	NE
	69	74	72	68	70·5	6	5	0	0	NE 0	NE
	68	74	75	70	71·5	0	0	6	0	NW 2	NW
	65	71	84	72	74·5	8	10	10	10	NW 1	NW
	68	71	90	75	79	10	10	10	10	W 1	NW
	71	76	81	74	76	10	10	10	10	NW 1	N
	70	74	88	71	79	10	10	10	10	NE 1	NE
	65	71	82	74	73·5	10	10	10	10	W 1	NW
	68	74	80	75	74	10	10	10	10	SW 1	SW
	70	76	82	75	76	0	10	10	10	S 1	SW
	72	77	80	73	76	10	10	10	6	SW 1	NW
	70	73	79	72	74·5	0	5	5	0	NW 0	NW
	69	74	83	70	76	0	6	6	0	NW 1	NE
	68	69	72	73	70	0	0	8	0	NW 0	NW
	71	74	76	71	74·5	0	6	8	0	NE 1	NE
	70	73	80	74	75	0	5	10	0	NE 1	NW
	70	74	86	75	78	10	10	10	10	N 0	NE
	69	76	79	75	74	10	10	10	10	SW 0	W
	69	76	86	76	77·5	5	5	8	0	W 0	W
	73	76	80	75	76·5	0	0	5	0	SW 1	SW
	70	73	74	72	72	0	0	5	0	NE 1	E
	70	74	79	74	74·5	0	6	8	0	NW 1	W
	70	76	86	73	78	5	10	10	0	NW 1	NW
	68.71	73·10	79·83	72·23	74·29	3·84	5·84	7	4·65		

CAL REGISTER.

f Barometer above low water, about twenty-five feet. Kept by JOSEPH P. RUSSELL, M. D., Medical Staff, U. S. A.

WIND.			CLOUDS.				WET BULB.		RAIN.			DANIELL'S HYGROMETER.		REMARKS.
9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	3 P. M.	Began.	Ended.	Quantity	Hygrometer.	Thermometer.	
							Fahr.	Fahr.			Inches.	Fahr.	Fahr.	
N 1	NE 0	NE 1	NE 5	NE 4	0	0	62°	72°				67°	75°	
NE 0	NE 0	NW 0	0	NE 4	0	0	63	75				64	76	
NE 0	NE 0	NE 0	0	NE 2	0	0	65	72				63	74	
NE 0	SE 0	SE 0	NE 2	NE 2	0	0	61	76				65	76	
NE 1	NE 1	NW 6	0	NE 2	NE 4	0	66	70	7 P. M.			66	78	
NW 1	NW 1	SE 1	NW 4	0	0	0	65	73		5 A. M.	2·00	65	75	
SE 0	SE 0	SE 0	0	0	0	0	63	72				61	72	
SW 1	NW 5	SW 1	0	0	NW 6	0	70	71	5½ A.M.	6½ P. M.	·03	58	72	
NE 0	SE 0	NW 1	0	0	0	0	66	68	3 P. M.	5 P. M.	·40	56	66	
NE 0	NE 1	NE 1	NE 5	NE 4	0	0	66	70		2 P. M.	·12	65	70	
NW 2	NW 1	NW 1	NW 2	NW 2	0	0	66	71	12 M.			66	68	
NW 2	W 1	NW 1	NW 2	0	0	0	64	78			2·50	65	76	
NW 1	SE 6	SW 1	0	0	0	0	66	85				62	84	
N 0	NW 3	NW 1	0	0	0	0	68	76				64	75	
NE 1	SW 1	SW 1	0	0	0	0	66	75			·03	66	79	
NW 1	SW 1	SW 1	0	0	0	0	63	76				62	78	
SW 1	SW 1	S 1	0	0	0	0	65	76				61	78	
SW 1	SE 0	SE 3	0	0	0	0	68	77				58	76	
NW 1	NW 1	NE 2	0	0	0	NE 2	67	75				55	74	
NW 1	NE 0	NE 0	0	NW 3	NE 2	0	68	76			·45	55	75	
NE 0	SE 0	SW 0	0	NE 2	0	0	68	78	4 P. M.			58	79	
NW 1	NW 0	NW 1	0	0	0	0	68	78		11 A. M.	9·70	50	68	
NE 0	NE 1	NW 1	0	NE 2	0	0	68	72				48	66	
NW 1	NW 1	NW 0	0	NW 4	0	0	66	76				54	75	
NE 1	NE 1	SW 1	0	0	0	0	65	80				60	78	
W 0	S 3	SW 1	0	0	0	0	66	76				58	78	
W 1	SE 3	SE 3	W 3	W 2	SE 2	0	64	75				61	74	
SW 0	SW 0	NW 0	0	0	0	0	68	75	9 A. M.	11 A. M.	·02	60	73	
E 0	SE 2	SW 1	0	0	SE 4	0	66	72			·01	58	70	
W 0	NW 1	NW 1	0	W 2	NW 2	NW 2	66	74				60	75	
NW 1	NW 1	NW 1	NW 3	NW 0	NW 0	NW 1	65	74				62	81	
							66·74	74·64			15·26	60·42	74·65	

METEOROLOGI

STATION, Fort Columbus, New York Harbor. Latitude, 40° 42′ North. Longitude, 74° 9′ West. Altitude of B

1843.	BAROMETER.				THERMOMETER ATTACHED.				THERMOMETER DETACHED.					CLEARNESS OF THE SKY.					
Sept.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily Mean.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A
	Centi-metres.	Centi-metres.	Centi-metres.	Centi-metres.	Centi-grade.	Centi-grade.	Centi-grade.	Centi-grade.	Fahr.	Fahr.	Fahr.	Fahr.	Fahr.						
1	76·31	76·31	76·30	76·29	19°	19°	19°	18°	68°	70°	68°	68°	68°	0	5	5	6	NW 3	N
2	76·31	76·31	76·31	76·31	18	19	26	22	66	68	80	74	73	0	0	8	6	NE 0	N
3	76·31	76·31	76·32	76·31	21	27	31	23	70	82	90	78	80	10	10	10	5	N 1	N
4	76·40	76·39	76·04	76·14	21	26	34	28	72	80	92	79	82	10	10	6	0	NW 0	N
5	76·04	76·09	76·19	76·24	25	33	33	26	72	78	91	75	81·5	10	10	10	10	NE 1	N
6	76·49	76·49	76·44	76·49	20	22	24	19	69	74	76	70	72·5	5	3	5	6	NE 2	E
7	76·59	76·59	76·64	76·59	17	18	20	18	64	66	68	67	66	4	3	0	6	E 2	E
8	76·54	76·54	76·49	76·49	18	18	22	17	66	67	74	65	70	0	8	8	10	NE 2	N
9	76·49	76·44	76·44	76·49	16	17	19	16	62	63	72	68	67	10	10	10	10	NW 5	N
10	76·49	76·44	76·44	76·39	13	14	15	13	55	60	64	56	59·5	0	6	4	0	NE 2	N
11	76·57	76·54	76·54	76·54	12	13	13	12	50	52	53	52	51·5	0	0	0	0	NW 0	S
12	76·54	76·54	76·54	76·54	12	14	18	15	52	54	64	56	58	5	6	10	0	NE 0	N
13	76·59	76·59	76·69	76·80	12	15	20	16	52	56	70	58	61	10	10	6	0	NE 3	E
14	76·89	76·89	76·84	76·80	12	13	16	15	54	60	62	60	58	3	5	0	0	E 0	N
15	76·60	76·40	76·40	76·40	19	20	23	22	70	71·5	74	70	72	2	0	5	0	SE 8	SE
16	76·19	76·18	76·15	76·15	20	26	28	24	70	73	76	72	73	10	10	10	10	SW 1	S
17	76·23	76·20	76·25	76·25	18	21	30	28	68	72	80	74	74	2	5	10	0	W 1	S
18	76·34	76·40	76·40	76·40	20	25	29	24·5	70	76	86	77	78	0	8	10	5	W 1	S
19	76·50	76·50	76·50	76·50	20	24	28	24	70	75	82	75	76	10	10	10	10	NW 1	N
20	76·48	76·65	77·25	77·10	20	22	26	22	70	74	82	72	76	10	10	10	10	NE 1	N
21	77·06	77·05	76·79	76·59	16	20·5	29	24	64·5	71·5	82	75	73·25	6	10	10	0	SW 5	S
22	76·25	76·20	76·25	76·30	17	17	22	20	60	62	78	70	69	10	10	10	0	NE 4	N
23	76·39	76·30	76·30	76·30	14	16·5	23	20	58	62	72	68	65	0	4	8	0	SW 0	S
24	76·20	76·40	76·40	76·40	18	19	26	22	60	68	80	74	70	6	10	10	10	SW 2	S
25	76·19	76·25	76·24	76·00	22	28	25	20	72	80	84	72	78	10	8	10	0	W 1	W
26	76·00	75·80	75·99	76·00	20	21	20	15	68	72	62	58	65	4	2	0	0	NW 2	N
27	76·04	76·10	76·14	76·20	8	13	14	7	48	53	60	42	54	10	6	10	0	NE 1	N
28	76·24	76·24	76·60	76·59	4	9	14	12.5	39	46	60	56	49·5	10	6	0	10	NE 1	N
29	76·55	76·60	76·65	76·50	11	19	25	14	52	64	68	55	60	10	10	10	0	NW 1	N
30	76·45	76·50	76·50	76·30	10	18	20	18	52·5	60	68	60	60·25	10	10	10	0	NE 0	N
Monthly Mean.	76·41	76·41	76·43	76·41	16·43	19·53	23·07	19·13	62·13	67·00	73·93	66·53	68·03	5·90	6·83	7·17	3·60		

METEOROLOGICAL REGISTER.

Latitude, 74° 9′ West. Altitude of Barometer above low water, about twenty-five feet. Kept by JOSEPH P. RUSSELL, M. D., Medical Staff, U. S. A.

CLEARNESS OF THE SKY.				WIND.				CLOUDS.				WET BULB.		RAIN.			DANIELL'S HYGROMETER.		REMARKS.
Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	3 P. M.	Began.	Ended.	Quantity	Hygrometer.	Thermometer.	
												Fahr.	Fahr.			Inches.	Fahr.	Fahr.	
0	5	5	6	NW 3	NW 2	NE 1	NE 0	0	NW 3	NE 3	0	66°	65°				58°	66°	
0	0	8	6	NE 0	NW 0	SW 2	SW 1	0	0	SW 2	SW 3	64	75				60	76	
10	10	10	5	N 1	N 1	SW 1	NW 1	0	0	0	NW 6	66	74				56	84	
10	10	6	0	NW 0	NW 1	W 2	SW 1	0	0	W 4	SW 1	70	85				54	85	
10	10	10	10	NE 1	NE 1	NE 2	NE 1	0	0	0	0	68	84				53	84	
5	3	5	6	NE 2	E 1	NE 1	NE 0	NE 4	0	NE 4	0	66	73				50	72	
4	3	0	6	E 2	E 1	SE 3	NE 1	E 3	E 2	0	NE 3	62	65	9 A. M.	4 P. M.	·10	48	66	
0	8	8	10	NE 2	NE 2	N 1	NE 1	0	E 3	0	0	65	72				50	71	
10	10	10	10	NW 5	NW 4	NE 2	NE 0	0	NE 2	0	0	60	59				48	60	
0	6	4	0	NE 2	NE 1	SW 1	NW 0	0	0	SW 4	0	52	58	7 P. M.			48	59	
0	0	0	0	NW 0	SW 1	NW 0	NE 1	0	NE 3	0	0	50	52	9 A. M.	11 A. M.	1·50	46	53	
5	6	10	0	NE 0	NE 0	NE 0	NE 0	0	0	0	0	51	62			·20	52	62	
10	10	6	0	NE 3	E 2	E 0	NE 2	0	0	0	0	48	65				49	66	
3	5	0	0	E 0	NE 2	NE 2	SE 2	0	0	0	0	52	61			·10	49	60	
2	0	5	0	SE 8	SE 7	SE 4	NW 1	SE 4	NE 4	SE 5	NW 0	68	72	9 A. M.	12 M.	·16	52	70	
10	10	10	10	SW 1	SW 1	W 1	NW 0	0	SE 2	0	0	68	73				50	71	
2	5	10	0	W 1	SW 1	SW 1	W 1	W 3	0	0	0	66	75				56	76	
0	8	10	5	W 1	SW 1	S 1	NW 1	0	SW 2	0	0	70	82				55	80	
10	10	10	10	NW 1	NW 0	NW 0	NW 0	0	SW 2	0	0	66	78				49	75	
10	10	10	10	NE 1	NE 0	NE 0	NW 0	0	0	0	0	67	76				51	78	
6	10	10	6	SW 5	SW 3	SW 2	NW 3	SW 3	SW 0	0	0	63	78				60	78	
10	10	10	0	NE 4	NE 2	NE 1	NW 0	0	0	0	0	60	76				58	75	
0	4	8	0	SW 0	SW 1	SW 0	SW 0	0	SW 2	0	0	56	70				58	71	
6	10	10	10	SW 2	SW 2	SW 0	SW 0	0	0	0	0	58	76				62	70	
10	8	10	0	W 1	W 1	SW 1	NW 0	0	0	0	0	60	78	6½ P. M.	7½ P. M.	·20	68	76	
4	2	0	0	NW 2	NW 0	NE 0	NE 3	NW 5	0	0	0	66	60	10 A. M.	11 P. M.	·80	59	61	
10	6	10	0	NE 1	N 2	NW 0	NE 3	0	N 3	0	NE 2	48	59				58	56	
10	6	0	10	NE 1	NW 1	SW 3	SW 4	0	0	SW 0	0	38	60				59	62	
10	10	10	0	NW 1	NW 0	NW 0	NE 0	0	0	0	0	48	66				58	68	
10	10	10	0	NE 0	NE 0	NE 0	NE 0	0	0	0	0	51	66				56	65	
5·90	6·83	7·17	3·60									59·70	69·63			3·06	54·53	69·87	

METE

STATION, Fort Columbus, New York Harbor. Latitude 40° 42′ North. Longitu

1843.	BAROMETER.				THERMOMETER ATTACHED.				THERMOMETER DETACHED.					
October.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily Mean.	Sui
	Centi-metres.	Centi metres.	Centi-metres	Centi-metres.	Centi-grade.	Centi-grade.	Centi-grade.	Centi-grade.	Fahr.	Fahr.	Fahr.	Fahr.	Fahr.	
1	76·20	76·20	76·04	76·00	15°	16°	20°	20°	50°	59°	60°	60°	55°	
2	75·90	75·94	76·00	76·00	15	17	23	16	60	64	72	53	66°	1
3	75·90	75·57	76·40	76·35	13	16	16	14	55	60	58	50	56·5	
4	76·32	76·34	76·34	76·30	9	11	14	12	46	50	52	48	49	1
5	76·30	76·31	76·36	76·36	10	16	26	18	48	50	60	55	54	1
6	76·39	76·30	76·25	76·20	10	20	24	18	50	65	70	60°	60	1
7	76·15	76·10	76·10	75·90	16	16	16	16	60	61	60	60	60	
8	75·50	75·55	75·60	75·60	18	18	17	13	63	62	60	56	61·5	
9	75·55	75·60	75·60	75·60	7	12·5	14	10	44	56	64	54	54	1
10	75·55	75·51	75·50	75·50	9	10	16	10	50	52	66	51	58	1
11	75·50	75·50	75·65	75·75	8	10	17	15	46	55	65	58	55·5	
12	75·85	75·90	75·90	76·00	12	16	22	14	52	58	72	60	62	
13	76·15	76·15	76·04	76·10	8	14	15	11	46	52	65	54	55·5	1
14	76·10	76·11	76·40	76·40	6	10·5	11	8	42	48	55	50	48·5	
15	76·50	76·48	76·64	76·60	4	8	11·5	9	40	44	53	50	46·5	
16	76·56	76·54	76·54	76·30	10	13·5	14	8	51	56	56	46	53·5	
17	76·19	76·14	76·00	76·00	5	10	11·5	9	40	50	54	48	47	1
18	76·00	76·00	76·25	76·09	6·5	10	14	12	42	48	60	54	51	1
19	76·16	76·49	76·59	76·54	9·5	11·5	15	10	48	53	59	52	53·5	1
20	77·00	77·10	77·00	76·90	6	10·5	13	12	47	52	65	56	55	1
21	76·19	76·14	76·09	76·04	15	16	20	17	59	61	74	63	66·5	1
22	75·80	75·65	75·60	75·60	15	16	13	13	60	63	58	46	59	1
23	76·30	76·30	76·51	76·48	8	8	10	7	42	48	52	45	47	
24	76·65	76·94	76·60	76·50	8	13	16	10	39	47	68	50	53·5	1
25	76·49	76·40	76·34	76·60	12	12·5	12	10	52	53	53	50	52·5	
26	76·70	76·70	76 66	76·45	8	8	8	8	48	52	49	44	48·5	
27	75·60	75·50	75·55	75·80	8	10	16	10	46	51	51	50	48·5	
28	76·70	76·70	76·65	76·60	10	10	12	8	45	45	45	45	45	
29	76·74	76·70	76·70	76·60	7	11	16	9	44	48	58	46	51	
30	76·50	76·50	76·50	76·78	8	9	10	7	45	45	47	44	46	
31	77·20	77·10	77·16	77·39	4	5	11	6	38	42	50	40	44	1
Monthly Mean.	76·21	76·21	76·24	76·24	9·70	12·40	15·10	11·60	48.32	53	59·06	51·58	53·66	

METEOROLOGICAL REGISTER

York Harbor. Latitude 40° 42′ North. Longitude 74° 9′ West. Altitude of Barometer above low water, about

ER ATTACHED.		THERMOMETER DETACHED.					CLEARNESS OF THE SKY.				WIND.				CLOU		
3 P. M.	9 P. M	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily Mean.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3
Centi-grade.	Centi-grade.	Fahr.	Fahr.	Fahr.	Fahr.	Fahr.											
20°	20°	50°	52°	60°	60°	55°	0	0	0	0	SE 0	SE 2	SE 1	SW 2	0	0	
23	16	60	64	72	53	66	10	10	10	0	SW 0	SW 0	NW 4	NW 3	0	0	
16	14	55	60	58	50	56·5	6	7	3	0	SW 1	W 5	NW 8	NW 8	SW 5	W 4	
14	1½	46	50	52	48	49	10	10	5	6	W 5	NW 4	NW 4	NW 6	0	0	
20	18	48	50	60	55	54	10	10	10	10	NE 6	NW 4	NW 1	NW 1	0	0	
24	18	50	65	70	60	60	10	10	10	10	NW 0	SW 0	SW 0	S 0	0	0	
16	16	60	61	60	60	60	0	0	0	0	SE 1	NE 1	NE 1	E 6	0	0	
17	13	63	62	60	56	61·5	0	0	0	8	SE 4	S 3	SW 2	SW 4	0	0	
14	10	44	56	64	54	54	10	10	10	10	NE 2	SW 0	W 0	NW 2	0	0	
16	10	50	52	66	51	58	10	10	8	6	NW 2	NW 2	NW 2	NW 1	0	0	
17	15	46	55	65	58	55·5	5	10	10	10	NW 2	NW 0	NW 1	NW 0	NW 3	0	
22	14	52	58	72	60	62	6	10	10	10	E 0	SE 0	NE 1	NE 2	0	0	
15	11	46	52	65	54	55·5	10	10	10	5	N 2	NE 0	NW 2	NW 4	0	0	
11	8	42	48	55	50	48·5	5	8	7	0	NW 3	NW 2	NW 3	NW 2	NW 2	NW 1	
11·5	9	40	44	53	50	46·5	5	4	0	0	NW 2	NW 1	SW 0	SE 0	NW 3	NW 4	
14	8	51	56	56	46	53·5	5	6	10	5	SW 2	SW 1	NW 1	NW 4	SW 4	SW 2	
11·5	9	40	50	54	48	47	10	10	2	0	N 3	NW 2	NW 4	NW 3	0	0	
14	12	42	48	60	54	51	10	10	0	10	NW 2	NW 3	W 3	NW 4	0	0	
15	10	48	53	59	52	53·5	10	7	10	10	NW 4	NW 3	NE 2	NE 1	0	NW 3	
13	12	47	52	65	56	55	10	10	10	10	SE 1	SE 2	SW 2	W 0	0	0	
20	17	59	61	74	63	66·5	10	10	10	10	NW 2	SW 2	W 0	NE 1	0	0	
13	13	60	63	58	46	59	10	10	0	0	NE 2	SE 1	SE 5	SE 4	0	0	
10	7	42	48	52	45	47	5	6	2	10	NE 0	NE 5	NE 5	NW 1	NE 4	NE 4	
16	10	39	47	68	50	53·5	10	10	10	10	W 1	SW 1	S 0	SE 0	0	0	
12	10	52	53	53	50	52·5	4	5	2	0	E 0	SE 0	SW 0	NW 0	0	0	
8	8	48	52	49	44	48·5	0	0	0	0	NE 1	NE 2	NE 1	SE 2	0	0	
16	10	46	51	51	50	48·5	0	0	0	0	SE 2	SE 1	N 2	N 0	0	0	
12	8	45	45	45	45	45	6	0	8	10	SW 1	SW 1	W 0	NW 0	SW 4	SW 1	
16	9	44	48	58	46	51	2	6	10	10	NE 1	NE 0	NE 0	NW 1	NE 3	NE 2	
10	7	45	45	47	44	46	5	6	0	10	SW 1	SW 1	SW 0	NW 0	SW 4	0	
11	6	38	42	50	40	44	10	10	10	10	NE 2	NE 2	NE 0	NE 0	0	0	
15·10	11·60	48.32	53	59·06	51·58	53·66	6·60	6·93	5·71	5·81							

ER.

about twenty-five feet. Kept by JOSEPH P. RUSSELL, M. D., Medical Staff, U. S. A.

CLOUDS.			WET BULB.		RAIN.			DANIELL'S HYGROMETER.		REMARKS.
A. M.	3 P. M.	9 P. M.	Sunrise.	3 P. M.	Began.	Ended.	Quantity	Hygrometer.	Thermometer.	
			Fahr.	Fahr.			Inches.	Fahr.	Fahr.	
0	0	0	52°	58°				54°	62°	Began to rain at 9 P. M. 30th September.
0	0	0	58	69		5 A. M.	2·00	56	65	Thunder and lightning during the night.
4	NW 4	0	53	58				50	60	
0	NW 5	NW 5	45	51				48	53	
0	0	0	46	56				50	58	
0	0	0	48	66				50	67	
0	0	0	58	58	6 A. M.	6 P. M.	1·50	52	59	
0	0	SW 3	60	58	7 P. M.	5 P. M.	1·30	50	59	Day stormy, with thunder and lightning.
0	0	0	42	60				50	58	
0	NW 2	NW 3	48	60				44	65	
0	0	0	45	54				46	63	
0	0	0	50	68				58	60	
0	0	NW 4	45	48				54	64	
V 1	NW 2	0	40	36				36	58	
V 4	0	0	44	47				37	52	
V 2	NW 1	NW 5	53	50				42	63	
0	NW 5	NW 3	40	50				41	56	
0	0	0	41	47				35	58	
V 3	0	0	46	50			·01	34	54	Rain during the night.
0	0	0	44	61				36	58	
0	0	0	47	68				39	68	
0	0	0	58	53				38	62	
4	NE 6	NW 1	44	40			·50	34	48	Rain during the night.
0	0	0	40	54				36	50	
0	0	0	48	48				38	50	
0	0	0	42	46				38	53	
0	0	0	45	44			·60	40	52	
1	W 2	0	44	41				42	51	
2	0	0	40	51				40	58	
0	0	0	42	43				39	48	
0	0	0	36	46				36	52	
			46·60	52·90			5·91	43·32	57·52	

STATION, Fort Columbus, New York Harbor. Latitud

1843.	BAROMETER.				THERMOMETER ATTACHED.				T
Novemb.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.
	Centi-metres.	Centi-metres.	Centi-metres.	Centi-metres.	Centi-grade.	Centi-grade.	Centi-grade.	Centi-grade.	Fahr.
1	77·70	77·60	77·60	77·20	3°	9°	13°	10°	34°
2	76·10	76·09	76·04	76·05	10	12	12	8	50
3	76·54	76·00	76·20	76·20	5	6	11	5	40
4	76·20	76·20	76·80	76·70	2	5	10·5	3·5	38
5	76·80	76·90	76·70	76·79	1	1·5	6	3	34
6	76·79	76·90	76·90	76·70	0	2·5	8	3	33
7	76·68	76·65	76·00	76·50	1	3	3	2	33
8	76·40	76·50	76·40	76·40	2	4	5	4	33
9	76·40	76·40	76·40	76·50	2·5	4	6	6	35
10	76·55	76·55	76·55	76·50	7	8	8	6	44
11	76·10	76·00	76·00	76·05	4	7	8	7	43
12	76·20	76·05	76·00	76·00	6	8	8	4	38
13	76·15	76·10	76·29	76·49	3	5	6	2	32
14	76·60	76·60	76·70	76·75	—1	3	3	2	31
15	77·00	76·95	76·90	76·90	—1	2	6	6	31
16	76·80	76·80	76·80	76·77	5	6·5	8·5	6	38
17	76·81	76·80	76·79	76·76	5·5	8	11	8	40
18	76·70	76·71	76·60	76·55	10	12	14	8	50
19	76·42	76·42	76·41	76·42	5·5	13	11	9	44
20	76·49	76·48	76·50	76·48	3·5	5	6·5	7	39
21	76·00	76·00	75·50	75·50	5	7·5	7·5	6	46
22	76·53	76·54	76·40	76·40	3.5	6	7	4	40
23	76·20	76·15	76·15	76·20	2	5	6	5	36
24	76·00	76·00	76·08	76·15	10	14	20	12	47
25	76·19	76·24	76·30	76·30	3·5	8	12	8	40
26	76·39	76·49	76·34	76·39	2·5	6	8	5	40
27	76·40	76·40	76·40	76·40	—3·5	1	2·5	2	32
28	76·44	76·45	76·40	76·40	—5	2	3	1	28
29	76·40	76·40	76·00	76·00	—1	2	0	—1	32
30	76·10	76·13	76·55	76·80	—1	2	2·5	—3	32
Monthly Mean	76·45	76·45	76·42	76·44	3·83	5·93	7·77	5·20	37·77

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METEOROLOGICAL

atitude, 40° 42′ North. Longitude, 74° 9′ West. Altitude of Barometer ab

THERMOMETER DETACHED.					CLEARNESS OF THE SKY.				WIND.			
nrise.	9 A. M.	3 P. M.	9 P. M.	Daily Mean.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9
hr.	Fahr.	Fahr.	Fahr.	Fahr.								
4°	48°	55°	48°	44·5°	10	10	10	0	NW 1	NW 1	SW 0	
0	53	56	49	53	0	8	6	10	E 1	NW 2	NW 0	
0	44	53	44	46·5	10	10	5	10	NW 5	NW 4	NW 5	
8	40	42	39	40	10	6	0	4	NW 2	NW 1	NW 2	
4	34	44	35	39	4	6	8	10	NW 1	NW 1	NW 1	
3	36	48	38	40·5	10	10	10	10	NW 2	NW 2	NW 2	
3	36	34	33	33·5	0	0	0	0	NE 0	NE 0	NE 0	
3	34	40	38	36·5	8	4	8	10	NW 0	W 2	SW 1	
5	40	42	40	38·5	10	10	0	0	NW 0	NW 0	NW 2	
4	45	46	45	45	0	0	0	0	NE 0	NE 0	NE 0	
3	46	47	45	45	0	0	0	6	SE 0	SE 0	SE 0	
5	42	43	34	40·5	10	10	6	10	NE 3	NW 6	NW 5	
2	34	35	32	33·5	6	10	4	10	W 2	NW 3	NW 2	
1	36	36	33	33·5	10	4	6	10	NW 3	NW 3	NW 1	
1	34	42	42	36·5	6	0	0	0	NE 1	NE 0	NE 0	
3	43	46	40	42	0	0	0	0	S 0	S 0	W 1	
0	46	52	48	46	0	2	0	0	N 0	E 1	SE 1	
0	54	58	48	54	0	0	10	10	SW 1	SW 1	SW 1	
4	51	57	49	50·5	10	6	8	10	W 2	NW 2	NW 4	
0	44	47	48	43	10	8	0	0	W 2	W 2	W 1	
6	48	47	46	46·5	0	0	4	4	E 1	E 1	W 8	
0	42	44	38	42	4	10	5	10	W 3	W 3	NW 6	
6	42	43	40	39·5	10	10	10	10	SW 3	W 1	NW 1	
7	48	56	46	51·5	0	0	0	10	SW 1	SW 1	SW 1	
0	46	50	43	45	10	10	10	10	NW 3	NW 2	NW 1	
0	44	48	43	44	0	4	0	10	NW 1	W 1	SW 3	
1	36	41	32	36·5	10	10	10	10	N 3	NW 3	NW 3	
3	36	34	32	31	10	10	10	6	NW 2	NW 1	NW 1	
2	34	32	31	32	0	0	0	0	W 1	NW 1	SW 1	
3	38	39	30	35·5	8	8	0	10	NW 1	NW 1	NW 1	
·77	41·80	45·23	40·30	41·50	5·53	5·53	4·33	6·33				

ERRATA.—In the Register for August—Barometer on the 8th, for 96·48 read 76·48; Thermometer attached, on t

TEOROLOGICAL REGISTER.

4° 9′ West. Altitude of Barometer above low water, about twenty-five feet. Kept by Jos

NESS OF THE SKY.			WIND.				CLOUDS.				WET BULB.		R	
A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Sunrise.	3 P. M.	Began.	En
											Fahr.	Fahr.		
10	10	0	NW 1	NW 1	SW 0	SE 0	0	0	0	SE 4	35°	50°		
8	6	10	E 1	NW 2	NW 0	NW 1	0	NW 4	NW 4	0	46	53		9 A
10	5	10	NW 5	NW 4	NW 5	NW 5	0	0	NW 2	0	38	50		
6	0	4	NW 2	NW 1	NW 2	NW 3	0	NW 4	0	0	36	40		
6	8	10	NW 1	NW 1	NW 1	NW 2	NW 3	NW 4	0	0	30	38		
10	10	10	NW 2	NW 2	NW 2	NE 0	0	0	0	0	30	45		
0	0	0	NE 0	NE 0	NE 0	NE 0	0	0	0	0	30	32	1 P. M.	5 I
4	8	10	NW 0	W 2	SW 1	NW 0	NW 2	W 3	SW 4	0	30	36		
10	0	0	NW 0	NW 0	NW 2	NE 0	0	0	0	0	31	39		
0	0	0	NE 0	NE 0	NE 0	NE 0	0	0	0	0	46	44		11 A
0	0	6	SE 0	SE 0	SE 0	NE 6	0	0	0	0	46	43	9½ A.M.	6 I
10	6	10	NE 3	NW 6	NW 5	NW 5	0	0	NW 6	0	36	41		
10	4	10	W 2	NW 3	NW 2	NW 4	W 2	0	NW 3	0	30	31		
4	6	10	NW 3	NW 3	NW 1	NW 2	0	NW 1	NW 4	0	30	34		
0	0	0	NE 1	NE 0	NE 0	NE 0	NE 3	0	0	0	29	38		
0	0	0	S 0	S 0	W 1	W 1	0	0	0	0	40	40		
2	0	0	N 0	E 1	SE 1	SE 1	0	0	0	0	42	42		
0	10	10	SW 1	SW 1	SW 1	SW 1	0	0	0	0	44	44	8 P. M.	
6	8	10	W 2	NW 2	NW 4	NW 2	0	NW 1	NW 2	0	40	50		
8	0	0	W 2	W 2	W 1	SW 1	0	0	0	0	38	40		
0	4	4	E 1	E 1	W 8	W 4	0	0	0	0	42	42		8½
0	5	10	W 3	W 3	NW 6	NW 4	W 3	0	NW 7	0	44	40		
0	10	10	SW 3	W 1	NW 1	NW 1	0	0	0	0	34	38		
0	0	10	SW 1	SW 1	SW 1	W 0	0	0	0	0	48	54		
0	10	10	NW 3	NW 2	NW 1	NW 1	0	0	0	0	37	50		
4	0	10	NW 1	W 1	SW 3	NW 1	NW 1	W 1	SW 2	0	36	40		
0	10	10	N 3	NW 3	NW 3	NW 1	0	0	0	0	30	32		
0	10	6	NW 2	NW 1	NW 1	NW 1	0	0	0	0	29	33		
0	0	0	W 1	NW 1	SW 1	NW 1	0	0	0	0	30	30	12 M.	4 P
8	0	10	NW 1	NW 1	NW 1	NE 1	0	0	0	0	30	30		
5·53	4·33	6·33									36·23	40·63		

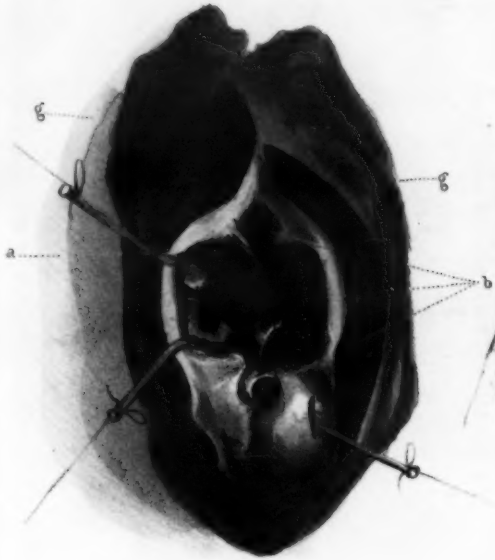
he 8th, for 96·48 read 76·48; Thermometer attached, on the 22d, for 53° read 23°; and on the 23d, for 24° at 9 P. M. read 21°.

y JOSEPH P. RUSSELL, M. D., Medical Staff, U. S. A.

RAIN.			DANIELL'S HYGROMETER.		REMARKS.
an.	Ended.	Quantity	Hygrometer.	Thermometer.	
		Inches.	Fahr.	Fahr.	
			34°	51°	
	9 A. M.	·50	46	52	Began to rain during the night.
			38	52	
			37	42	
			36	42	
			36	48	
M.	5 P. M.	·08	39	35	First fall of snow.
			35	41	
			36	43	
	11 A. M.	·10	48	46	Rain began in the course of the night.
.M.	6 P. M.	·60	42	46	Very high winds during the night.
			32	44	
			33	35	
			34	36	
			34	41	
		·55	44	46	Rain began and ended during the night.
			40	52	Dense fog until 8 o'clock A. M.
M.		·08	42	56	Rain began at 8 P. M. and ended during the night.
			40	56	
			44	57	
	8¼ A.M.	·85	47	47	Rain began in the course of the night. Severe squall began at 3 P.M., ended at 3¼ P.M.
			44	45	
			44	44	
			45	55	
			44	50	
			40	47	
			32	42	
			36	34	
M.	4 P. M.	·06	44	32	Sleet.
			27	38	A large halo around the moon.
		2·82	39·10	45·17	

Dr HOSACK'S CASE OF TUMOR OF PAROTID GLAND.

1.



2.

a b c & d *Melanotic matter.*
e & f *Scirrhous remains of Parotid gland.*
g g *Integument.*

LITH OF ENDICOTT.

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ART. XIII.—A Practical Treatise on Midwifery. By CHAILLY, Doctor of Medicine and Ex-chief of the Obstetric Clinique of the Faculty of

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Art. XIV.—Principles of Medicine: comprising General Pathology and Therapeutics, and a brief general view of Etiology, Nosology, Semeiology, Diagnosis, and Prognosis. By Charles J. B. Williams, M.D., F.R.S., Professor, etc. With additions and notes, by Meredith Clymer, M.D., etc. Philadelphia: Lea & Blanchard. 1844. 8vo., pp. 384.

The same "with numerous additional notes, explanatory and critical, by the Editor of the Select Medical Library and Bulletin of Medical Science." Philadelphia: Edward Barrington and George D. Haswell. 1844. 8vo., pp. 396., ... 260

Art. XV.—On the Nature and Treatment of Stomach and Renal Diseases; being an Inquiry into the connection of Diabetes, Calculus, and other Affections of the Kidney and Bladder, with Indigestion. By William Prout, M.D., F.R.S., &c. From the Fourth Revised London Edition, with Plates. Philadelphia: Lea & Blanchard. 1844. 8vo., pp. 466, ... 262

Art. XVI.—A Treatise on Dislocations and Fractures of the Joints. By Sir Astley Cooper, Bart., F.R.S., Sergeant-Surgeon to the King, etc. A new edition, much enlarged, edited by Bransby B. Cooper, F.R.S., Surgeon to Guy's Hospital. With additional Observations, and a Memoir of the Author. Philadelphia: Lea & Blanchard. 1844. 8vo., pp. 499, ... 264

Art. XVII.—The Principles and Practice of Medicine. By John Elliotson, M.D., Cantab., F.R.S., President of the Royal Medical and Chirurgical Society; President of the Phrenological Society; Late Professor of the Principles and Practice of Medicine, and of Clinical Medicine, and Dean of the Faculty of Medicine in University College, London; Late Senior Physician to the University College-Hospital: Fellow of the Royal College of Physicians; Formerly Physician to St. Thomas's Hospital; and President of the Royal Medical Society of Edinburgh. Edited by Nathaniel Rogers, M.D., etc., and Alexander Cooper Lee. First American, from the second London Edition, greatly enlarged and improved. With Notes and Additions by Thomas Stewardson, M.D., etc. Philadelphia: Carey & Hart. 1844. Large 8vo., pp. 1046, ... 266

Art. XVIII.—Lectures on the Principles and Practice of Physic; delivered at King's College, London, by Thomas Watson, M.D., etc. Philadelphia: Lea & Blanchard. 1844. 8vo., pp. 914, . . . 267

Art. XIX.—Medical Clinic: Diseases of the Chest. By G. Andral, Professor to the Faculty of Medicine of Paris, etc. Condensed and Translated, with Observations, by D. Spillan, M.D., etc. Philadelphia: Edward Barrington & George D. Haswell. 1843. 8vo., pp 408, 268

Art. XX.—Practical Medicine: Illustrated by Cases of the most important Diseases. Edited by John M. Galt, M.D., Philadelphia: Edward Barrington & George D. Haswell. 1843. 8vo., pp. 328, . ib.

Art. XXI.—Lecture Introductory to a Course on Chemistry in the University of Pennsylvania, delivered November 7th, 1843. By Robert Hare.

Introductory Lecture to the Course of General, Descriptive, and Surgical Anatomy, in Jefferson Medical College, delivered November 8th, 1843. By Joseph Pancoast, M.D.

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ERRATA.

Page 184, line 5 from bottom, for *morbification* read *morbid action*.
" 203, " 24 " " for *simple* read *single*.
" 215, " 19 " top, substitute for the stars, the word *Like*.
The last two errors occur only in part of the edition.

TO READERS, CORRESPONDENTS, ETC.

We would claim the indulgence of such of our contributors, whose articles have been necessarily deferred; and we would also ask authors and publishers to look with forbearance upon similar delays as regards the notices of books. Even as it is, we have, in this number, encroached unwarrantably upon Part III.

Contributions have been received from Professor Caldwell, Dr. King, U. S. A., and Dr. George M. Maclean; and we would take this opportunity to add that the contributor in our last number, who published the same article simultaneously in another journal, exceeded, in our opinion, the bounds of propriety.

We have to acknowledge the receipt of the following works:

A Lecture Introductory to the Course of Obstetrics in Geneva Medical College, Session of 1843–4. By C. B. Coventry, M.D.

A Lecture Introductory to a Course on Chemistry, in the University of Pennsylvania. Delivered Nov. 7th, 1843. By Robert Hare. (From the Author.)

An Essay towards a correct Theory of the Nervous System. By John Harrison, M.D., Professor of Physiology and Pathology in the Medical College of Louisiana. Philadelphia: Lea & Blanchard. 1844. (From the Publishers.)

Lectures on the Principles and Practice of Physic; delivered at King's College, London. By Thomas Watson, M.D. etc. Philadelphia: Lea & Blanchard. 1844. (From the Publishers.)

Annual Report of the Surgeon-General to the Secretary of War. October 31, 1843. (From the Surgeon-General.)

Principles of Medicine, etc. By C. J. B. Williams, M.D., F.R.S., etc. With numerous additional notes, explanatory and critical, by John Bell, M.D., etc. Philadelphia: Ed. Barrington & Geo. D. Haswell. 1844. (From the Publishers.)

Report of the Trustees of the Maine Insane Hospital. Nov. 30, 1843.

Introductory Lecture of Joseph Pancoast, M.D., Professor of Anatomy in Jefferson Medical College. Session of 1843–4.

Pharmacologia, being an extended Inquiry into the Operations of Medicinal Bodies, upon which are founded the Theory and Art of Prescribing. By J. A. Paris, M.D. Cantab., F.R.S. With Notes by Charles A. Lee, M.D, A.M., etc. New York: Harper & Brothers, 82 Cliff street. 1844. (From the Publishers.)

The Anatomy and Surgical Treatment of Abdominal Hernia. With numerous plates. By Sir Astley Cooper, Bart., F.R.S., etc. From the second London edition. By C. Aston Key, Senior Surgeon of Guy's Hospital, etc. Philadelphia: Lea & Blanchard. 1844. 8vo. pp. 428. (From the Publishers.)

Catalogue and Circular of the Albany Medical College. 1844.

Catalogue of the Medical Students and Graduates of the Fall Session, at Castleton, 1843, and Annual Announcement of the Spring Session, 1844.

Annual Report of the Managers of the State Lunatic Asylum. Made to the Legislature of New York, January 18, 1844. (From A. Brigham, M.D.)

The same from T. Romeyn Beck, M.D.

Memorial, signed "D. L. Dix," to the Legislature of the State of New York, relative to the indigent and pauper insane persons, in the county-houses of the State. (From T. Romeyn Beck, M.D.)

Catalogue of the Faculty and Students of Jefferson Medical College, for the Session of 1843–4.

Catalogue of the College of Physicians and Surgeons of the City of New York. January, 1844.

Our space will only allow a condensed acknowledgment of the receipt of the following: Brande's Dictionary, Magendie's Physiology, Chailly's Midwifery, Chapman on the Thoracic and Abdominal Viscera, Smith's Anatomical Plates, and Webster's Introductory Lecture.

The London Medical Gazette. (In exchange.)

The American Journal of the Medical Sciences. (In exchange.)

The Boston Medical and Surgical Journal. (In exchange.)

The Medical Examiner. (In exchange.)

The St. Louis Medical and Surgical Journal. (In exchange.)

The Bulletin of Medical Science. (In exchange.)

The American Journal and Library of Dental Science. (In exchange.)

☞ Postmasters, it will be borne in mind, are authorized by law to frank letters containing money for the subscription of periodicals.

*** For Prospectus see fourth page of the cover.